Vol. 24, No. 5 May, 1927

THE
PSYCHOLOGICAL BULLETIN

PSYCHOLOGY AND CULTURE

BY MALCOLM M. WILLEY
Dartmouth College
and
MELVILLE J. HERSKOVITS
Columbia University

With the development of what has come to be known as cultural anthropology, and the application of its findings to sociology, there has been gathered a vast amount of material both factual and theoretical, that is of importance to the psychologist because of the bearings it has upon the problems of human behavior. Man has from times long past sought explanations for his own social and individual conduct, and there have developed innumerable theories to account for it: geographical theories, biological theories, abstract sociological theories, all of which have contributed something towards an understanding of human behavior. From all of these theories the psychologists have been able to draw material of value to themselves in connection with their specific problems. In more recent times the anthropologists have been gathering data from peoples with cultures less complex than our own, and interpreting them, in doing which material of great importance to psychology is made available.[1] It is the purpose of this paper to summarize the impor-

[1] Professor Knight Dunlap, for example, in a paper at the 1926 Philadelphia meeting of the A.A.A.S., Section I, emphasized the importance of the findings of anthropology for psychologists. In this paper, *The Fostering of Anthropological Instruction in Universities and Colleges*, he suggested that in institutions where no anthropology is now given it might well be developed in connection with existing departments of psychology, so closely are many phases of the two subjects related.

tant findings of the anthropologists, to show the development of their concepts which have most significance for psychology, and to enumerate some of the more important publications, especially of the last decade, in which this material may be found. The bibliography appended makes no pretense at completeness; it has been designed, rather, to include some of the more important contributions bearing upon the points discussed in the text of this presentation. Readers wishing to compare this with other survey articles are referred to the work of Barnes (6), Goldenweiser (39, 35, 40), Malinowski (81), and our own earlier contribution (134).

It may be well at the outset to indicate that the word anthropology has a different meaning on the European continent than in England and the United States. In the former countries it covers what would in America be classed as physical anthropology, ethnology being used to cover the study of cultural data, while in America and England the word *anthropology*, as explained by Kroeber (66), Wallis (132), Wissler (136) covers both the physical anthropology and the ethnological material,—the study of customs and habits. As Kroeber (66) points out, the field of anthropology embraces the study of the somatic characteristics of man, his social habits and customs (including the study of linguistics) and his pre-history, for archaeology offers much that is important to anthropology. Although physical anthropology holds much for the psychologist, particularly for those psychologists engaged in work with psychological tests, as may be illustrated by a paper by Murdock and Sullivan (93), and another by Herskovits (57), it is social anthropology, or the study of the *culture* of peoples, with which the present discussion is primarily concerned. The material bearing upon this subject can be treated as falling into four divisions, although it must be understood that such a process of dividing the consideration of culture cannot possibly present a picture of actuality, but, rather, is forced by inability otherwise to attain the necessary clarity. The divisions which we shall use, therefore, are first, the definition of culture; second, the morphology of culture; third, the mechanisms of culture; and fourth, the methods in the study of culture.

1. *The Concept of Culture.* Tyler (126) has given a classical definition of culture which bears repeating here: "that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society." This definition has been elaborated in many ways by Goldenweiser (36), Kroeber (66), Lowie (76), Ogburn (98), Sapir (111), Tozzer (125), Wallis (132), Willey and Herskovits (134) and

Wissler (145). Ogburn (98) has perhaps been as successful as any of these writers in drawing the distinctions between man's cultural possessions and his inborn biological equipment. Wissler (136) has stressed culture as a "mode of behavior" and with Boas (18) has pointed out certain difficulties involved in its study. Goldenweiser (36) by the use of concrete descriptions of certain phases of primitive life makes the concept clear, and further points out, as do Ogburn (98), Willey (135) and others, that the term culture is synonymous with what the sociologists have called social heritage, or even civilization.

How important it is that the psychologist, as well as other students of human behavior, should distinguish between that part of man's behavior which is explicable in terms of his cultural heritage, and that part which results from inborn tendencies, is developed by Ogburn (98) in much detail, and is also succinctly stated by Kroeber (66) and further elaborated by him in an article (62) upon which Ogburn and other students of cultural determinism have leaned heavily. In fact to Kroeber (66) "the specific task and place in the sun for anthropology" is "the interpretation of those phenomena into which both organic and social causes enter." Clearly, before such interpretation can be undertaken there must be in the mind of the investigator a distinct conception of what culture is, and where the line between it and the biological factors is to be drawn. As Boas (18), Lowie (76), Ogburn (98), and a host of others have shown, much error in psychological and sociological analysis has crept in through failure to separate these two factors. This is particularly true in the analyses where the results of psychological tests are utilized to interpret race differences. Such studies as those of Brigham (24), Grant (44), Josey (59), McDougall (80) and Stoddard (120) have been severely attacked by the students of culture on the ground that they have neglected to give weight to the cultural differences that are involved. For, if culture is a manifestation of the activities of man which has, more or less, a trend of its own, apparently one which acts as a conditioning mechanism for the individuals who live under it, then it is obvious that to the extent to which this is true the results of the "intelligence" tests are vitiated. Thus, it has been pointed out by Boas (22), Herskovits (53), Mead (90) and others that in the case of immigrants to this country, to claim, as Brigham did, that length of residence and acculturation to the customs of this land have little or nothing to do with standing in psychological tests based on experiences which are essentially functions of the culture of America, seems, from a cultural point

of view, unjustifiable. Lowie (78) has put it concisely: "Letting H and E represent heredity and environmental determinants, respectively, the empirical results may be formulated as follows:

$$H_1 + E_1 = A \text{ [Behavior]}$$
$$H_2 + E_2 = A \pm m$$

It does not require a profound knowledge of mathematics to see that the difference $\pm m$ proves nothing as to the value of H_1 and H_2 so long as E_1 and E_2 differ by an unknown quantity." The case with the Negroes is similar, since almost unanimous findings have shown that American Negroes stand lower in psychological tests than their white fellows. But again we have the cultural background to consider. Long (71) has presented a criticism on both the statistical method and the assumptions underlying the results, bringing out the interesting point, by implication, that it may be the cultural background of the psychologists themselves which has conditioned them to a point of view which would be congenial to such results regarding the Negro, while Clark (25) and Herskovits (57) have adduced data which show that where the educational and social background is at all comparable to that of whites, the results of testing do not differ significantly. Other anthropologists (Goldenweiser, 36, 37; Kroeber, 66; Lowie, 78; Tozzer, 125; Wallis, 133, to name only a few) have also called attention to these errors involved in the interpretation of results derived from giving intelligence tests. In short, the students of culture seriously question the results of much of the earlier sociological and psychological analysis of social behavior. Hence it is important to have a thorough understanding of the term culture as used by this group before undertaking a discussion of their work.

II. *The Morphology of Culture.* Wissler (144, 145) has gone furthest in presenting a systematic analysis of culture, and in demonstrating the manner in which any civilization—that of the most primitive as well as our own—may be broken up into its units for purpose of study. The analysis he presents is useful in the study of culture, although it must once more be reiterated that to break up culture into more or less artificial parts makes for a mechanical view of it. While it cannot be denied that for pedagogical and analytical purposes this schematization is excellent, at the same time the intangible values of culture, those values which make the same trait in two different cultures quite different in the part they play in the life of the people in the two cultures, must also be stressed. Indeed, as will be discussed later, the "functional" school, a recent develop-

ment centering in the persons of A. Radcliffe-Brown (**101**), and Bronislaw Malinowski (**81**), insists that it is only these values which are of any importance. They feel that it is only through a knowledge of the manner in which the aspects of a culture are interrelated, and of the psychological interplay between the culture and the minds of the individuals who live in it, that we can come to a comprehension of culture processes. (See particularly Malinowski, **81**) It is, of course, possible that in the reaction these men feel to the excess of data dealing with material culture and the plotting of distribution maps for various traits, they have gone somewhat too far in their emphasis. Certainly the work of Boas and his students has tended to include both points of view as will be seen later in this discussion. However, the functionalists cannot but have salutary effect in directing attention to the less striking, intangible aspects of social life, and as a corrective to an excessive addiction to the use of the somewhat formalized analysis of culture such as is necessarily the result of a summary presentation such as this.

The unit of any civilization according to Wissler's system is a "trait." To understand a culture thoroughly, he tells us, there must be an almost complete enumeration of the traits that in combination constitute the civilization. In fact, he states that "a culture is not to be comprehended until the list of its traits approaches completeness." However, again, the functionalists would question, and rightly, the absence of the "plus-quantity" given by the fact of the interplay of these traits after they had been catalogued. It should be borne in mind that cultural traits, as Ogburn (**98**) has so carefully explained, may be either material or non-material. A cheer at a football game constitutes a trait no less than does the ball with which the game is played (Willey, **135**). How vast is the variety of traits in any culture is shown by Wissler (**140**) in one of his studies of North American Indian life, and the overwhelming number of traits comprising our own culture is made apparent by Marin (**87**). To grasp thoroughly the concept of the culture trait, and to obtain an understanding of the interrelation of traits in a culture, one should read in detail some of the elaborate studies of primitive life compiled by such men as Boas (**10, 11, 13, 17**) for North America; Nordenskiöld for South America (**95**); Spencer and Gillin for Australia (**118**); Smith and Dale for Africa (**114**); Bogoras (**23**) for Siberia; and Malinowski (**82**) for Melanesia, to mention only a few more or less at random. The concept of the trait is also made clear by Goldenweiser (**36**). Benedict (**9**), Boas (**19**), Herskovits (**51**), Lowie (**79**), and Wissler (**144, 145**)

show that each trait of culture has its own history and that it is out of these disparate units that culture is compounded, a point of importance to bear in mind in the subsequent discussions of diffusion and pattern. The significance of this, as well as an enumeration of the characteristics of culture traits as summarized from the anthropological literature, is given by Willey (135). Benedict writes (9): "It is, so far as we can see, an ultimate fact of human nature that man builds up his culture out of disparate elements [traits], combining and recombining them; and until we have abandoned the superstition that the result is an organism functionally interrelated, we shall be unable to see our cultural life objectively, or to control its manifestation." The latter part of this sentence does not mean that Dr. Benedict is in opposition to the functionalist school mentioned previously, or that she argues that there is not an interrelation of traits within a culture. What is implied is rather that any given single trait may be taken by an extraneous culture, and this without taking any of the traits which were associated with it in the culture from which it was borrowed.

However, Lowie pointed out (79), as the functionalists have since emphasized, that mere enumeration is not enough, for two tribes might show a similar inventory of traits, and yet differ fundamentally in their cultures because of different ways of combining the different traits. Individual traits, as Wissler (145) demonstrates, tend to cluster, and to such a cluster of traits the name *complex* is given, and these culture-complexes form valuable keys to a more complete description of the cultures with which the anthropologists deal. Benedict (9), Hallowell (47), Herskovits (51) and Lowie (79) have shown that the traits in the complex bear no necessary organic relationship, and they develop the process of association by means of which the traits have been brought together. Wissler (144) has made a study of the distribution of the various traits in the Americas, in which he shows that trait complexes do not have an indiscriminate distribution, but that regularity of distribution can be perceived when localities where traits are found are plotted on a map. His studies of the maize complex (139, 142) and the spread of the horse culture on this continent are valuable contributions because of the clarity they give to the culture complex concept. It can be further shown that when a number of fundamental complexes are thrown together on a single map there is a significant overlapping, from which fact grows the concept of the *culture area*, as used by the American anthropologists. For this culture area concept we are indebted to Franz Boas; the history of the

concept as well as some indication of the debt of anthropological science to Boas has been outlined by Goldenweiser (35, 39, 40). Wissler (144, 145) and later Herskovits (56) have pointed out that within any area the cultures of the peoples who live in it may be subsumed, by the student, under a generalized type, from which form there are progressively larger variations as one proceeds away from the geographical culture center. (See Wissler, 145.) The culture area concept is objective to the extent that it is based upon the distribution of specific traits, and subjective in that it also involves the relationships between parts of the culture within the area as well as the attitude of the individuals toward the elements of their culture. (Goldenweiser, 35). That the culture area concept is not limited in application to the Americas has been proven by Herskovits (50, 56), who has further presented theoretical discussion of the concept contrasting it, especially, with the *Kulturkreise* concept of the German *kulturhistorische Schule.*

The work of this latter school differs in one important respect from that of the American anthropologists in that there is a complete neglect of the psychological elements involved. Graebner, Foy, Pater Schmidt, Ankermann, and others who belong to this group, feel that culture elements can be discovered or invented once only, and that lack of ability to see where historical connection between two cultures having similar traits might have occurred, or the geographical separation involved, should not stand in the way of postulating such connection. Graebner (42) has mapped, from his point of view, culture areas for Melanesia and Oceania (41, 43), while Ankermann (3) has done the same for Africa, and Pater Schmidt (112), who continued working along the same lines with the South American data, has more recently given, in a linguistic work, a map of "*die ethnologischen Kulturkreise*" which covers the entire world (113). Graebner insists that for a single trait of a culture complex to travel is "*kulturgeschichtliches Nonsens*" (42). Therefore, to map a culture area involves only noting on a map those places where the elements of the cultural complex are found, and the resulting area constitutes the *Kulturkreis.* On the other hand, while "the culture area" of the American school "like the *Kulturkreis,* posits diffusion, . . . it insists that more than mere logically comprehensible categories be maintained . . ." (Herskovits, 56). The essential difference between the two groups is a psychological one, and although both the American anthropologists and the German ones of the *kulturhistorisches Schule* call themselves historical in their method, the American workers insist on something nearer to actuality than log-

ical likelihood in ferreting out historical connections, and are firm in the conviction that the study of any cultures which does not take into account the psychological factors which make up the daily life of the people living under those cultures must be, in the end, sterile.

In any event, the importance to the student of behavior of a knowledge of these processes and arrangements of culture is apparent. If culture is considered as one phase of the environment to which man adjusts himself, as Ellwood (**26, 27**), Herskovits (**51**), Ogburn (**98**), Willey (**135**), and other sociologists stressing the cultural viewpoint have insisted it should be, then many of the differences in social behavior that are observable are explicable in terms of different cultures to which members of different groups have been conditioned. The conditioning process and its relation to culture has been fully elaborated by Wissler (**145**), and his analysis agrees with that of Boas (**18**) and his followers, who, making due allowance for individual differences, urge that the culture within which the individual is born and grows is an all important factor in shaping his individual and social behavior. (See Lowie, **78**.)

In working with the American material, Wissler (**140, 144, 145**) has established and delimited the culture areas of the continent, while Kroeber has done the same intensively for California (**64, 67**), and Herskovits has similarly mapped the areas of Africa (**50**). In dealing with these data it was found that there is a shading of the culture from one area to that of the next. To the region in which there is the overlapping of complexes, or where the culture becomes less "typical" through dropping out of traits, the name *marginal area* is given. (See also Goldenweiser, **35, 36, 39**; Herskovits and Willey, **134**; Kroeber, **62**.) While objectively the culture of the marginal region may be less developed, as Goldenweiser makes clear (**36**), to the individual residing in the area it has as much subjective reality as has the culture of the center of the area to the individual residing at the center.

The analysis, however, is carried still one step further; in fact, the concepts developed thus far would be regarded chiefly as preliminary. With traits, complexes, and geographical areas within which these localize, there still remains to consider the *form* of the culture within the area—the manner in which complexes are interrelated, and the importance they assume in the lives of individuals. It is consideration of this problem that leads to the concept of culture *pattern*, perhaps the most important aspect of this section of our discussion for the psychologist. It is developed in nearly all the work of the American students, but is best explained or exemplified

in such works as those of Benedict (9), Goldenweiser (30, 31, 35, 36, 39), Herskovits (51, 52, 56), Lowie (73, 74, 77), Spier (119), and Wissler (140). Bartlett has shown the relation of the pattern of the society to the process of borrowing (7), while the functionalist Malinowski also knows and by implication accepts the pattern concept (85, ch. VII). In all of this work the patterns are conceived as constituting a cast into which the culture has arranged itself, in which the various phases of the culture assume differing degrees of importance in the lives of the people who live under it (147). For, as is clear in all of this work, the mere form or arrangement of the culture is only one part of the analysis; the attitudes of the individuals within the pattern to their culture are equally important. Wissler has shown this (145), analyzing our own civilization both from the objective standpoint of culture area, as well as from the psychological processes involved in becoming conditioned to the culture, while Radin has edited a striking study involving the latter phase of the problem (103), and Herskovits has examined Harlem Negro culture from this pattern viewpoint (52). The sociological significance of the concept has not been worked out in detail by the anthropologists; their interests have been centered in the primitive peoples. However, they all appreciate the value of their concept for the study of contemporary civilization. Sociologists have begun to see how the anthropological analysis can be utilized, and have attempted to make suggestions along these lines (Ogburn, 96; Thomas, 122, 123; Thrasher, 124; Willey, 135). In many ways the work of William Graham Sumner (121) anticipated this type of analysis, and his folkways and mores are strikingly similar to *pattern* as conceived by the anthropologist. As Willey and Herskovits (134) suggest, the whole concept of social control might well be recast in the terminology of social pattern.

Wissler (145), continuing his analysis of the structure of culture, observes further that if all cultures, wherever found, be closely examined, it will be seen that all of the traits and complexes of which they are composed may be grouped under nine major headings, to which series of headings he gives the name "universal culture pattern." This is perhaps an unfortunate term since it may readily be confused with pattern in the sense of *the pattern* of any given society, nor is it an indispensable concept, for in the final analysis it merely means that we give similar chapter heads to all our ethnographic works, whatever tribe may be studied. Further, "it might be wished that he had selected a different term for his concept, for it would seem that the pattern of a civilization includes just those

elements that make that civilization stand out as distinct from others" (134). A theoretical germ plasm basis of this universal pattern is suggested by Wissler (145), man everywhere being conceived as a culture building animal; the differences between cultures then arise through different elaboration within the limits set by the universal pattern. However, this point, though interesting, cannot be substantiated as yet by biological investigation.

Thus far it might be gathered, in spite of the warnings we have given to the contrary, that culture is static, and that the American anthropologists whose work has been described are presenting their material in schematic and formal fashion. Such is not true. It is only the need for making the concept of culture stand out sharply that has led us in this survey to so analytical a presentation. Culture is essentially a dynamic thing, and is so regarded by the students of it (19). This will become clear if we now turn to discuss the mechanisms of culture.

III. *Mechanisms of Culture.* Whatever the validity of the biological implications of his theory, Wissler is quite correct in his statement that man is a culture building animal. Therefore any consideration of culture must include a discussion of the dynamics whereby culture is developed. That culture accumulates is scarcely to be doubted, and this fact may be set down as one of its outstanding characteristics. Kroeber (62, 66), Ogburn (98), Tozzer (125), and Wissler (145), among others, have attempted to show that from simple beginnings in the Stone Age the total of human possessions has been built up through a process of accretion. Ogburn suggests that this process of growth bears certain resemblances to the development of a compound interest curve (98), which further suggests that little if anything is lost in the process of accumulating. To the sum total of a culture is given the name culture base (98). While, as Rivers (107) has proved, occasional traits may be lost from a given culture, such loss in all probability is rare. To Ogburn and many students the size of the culture base—the extent of the accumulations—has particular importance in that the rate of growth, they argue, is conditioned by it. To them inventions thus become, within limits, inevitable (Ogburn, 98, 99; Ogburn and Thomas, 97), and the whole process of social control becomes questioned. This position was pronounced and elaborated by Kroeber, who says, concerning the development of culture (63): "It can well be argued on theoretical grounds that the greater or less innate capacity of this or that individual, or of any limited number of individuals, is of negligible consequence." Thus the individual comes within the grip of

his culture (Ogburn, 99; Wallis, 127). To be sure, culture grows only through man, but in culture itself, and not in man, is the causative factor that conditions the rate of growth (Kroeber, 62; Ogburn, 98, 99; Wallis, 127, 129). This whole position has been vigorously attacked both by sociologists and psychologists who feel that in final analysis man is the causative factor, and that the objectification of culture to the extent that it is carried by these extremists is fallacious (Allport, 1, 2; Hankins, 48). And again, many anthropologists refuse to force such cultural fatality to its logical conclusion. An interesting contribution bearing on this point has been made by Judd (59a), who in his analysis of social institutions stands in a position opposite to that of Ogburn.

Another characteristic of culture traits, mobility, underlies the most argued phase of culture theory, namely, problems of diffusion. Diffusion is the spread of culture traits and, as Wissler (140, 146) and Bartlett (7) have demonstrated, this spread may be either conscious or unconscious. Bartlett (7) has probably given the most comprehensive statement of the psychological factors that must be considered in studying changes and spread of culture. In much of the culture theory the material is developed in such a manner that there is danger in conceiving the spread of culture as something that goes on independent of man—that is, culture is all but attributed with independent mobility, which, of course, it does not possess. It is Bartlett who points to the need for an understanding of the psychological aspects of diffusion (7), and who tries to show the circumstances under which culture spreads, and the conditions necessary to its spread. This again introduces the subject of culture pattern, for the acceptance of a diffusing trait or complex (and either may spread) depends upon the possibility of adjusting it into the pattern of the group to whom the trait is spreading (Bartlett, 7; Willey and Herskovits, 134). Each trait gained from a foreign group is absorbed so as to conform to the general pattern of the society taking it (107), and if the trait is not adaptable to the pattern it will in all probability be rejected.[2] This has been amply shown by Benedict (9), Kroeber (60), Lowie (74), Radin (102), Wissler (141), and other students who have made detailed studies of phases of culture of American Indian life. Wissler has demonstrated the principle in his study of the spread of the horse in North America (139). The horse was readily adopted into the hunting pattern of the plains

[2] Boas (18) has utilized this line of reasoning in seeking to answer the frequent question: "If other races are equal to us, why don't they adopt our culture when they come into contact with it?"

Indians, but was not accepted by the Indians of the woodland regions who had little or no use for it. The same principle is elaborated by Benedict (9), who shows how the guardian spirit concept as it spread throughout North America changed from tribe to tribe to conform to the various religious patterns of the several tribes. The psychological basis for the phenomenon has been discussed by Bartlett (7), Boas (18), Lowie (79), and Wissler (145), who developed the doctrine of association to explain many of the aspects of diffusion. Lowie's discussion is perhaps the most complete, and is filled with illuminating examples of the mechanism involved. Here is one of the points where psychology and anthropology touch intimately. The anthropologist must fall back upon psychological principles for an interpretation of his data. Psychology may not explain, but it may give insight. Moreover, these discussions of pattern, and the correlated studies of diffusion with the acceptance or the nonacceptance of traits illustrate, as Lowie clearly states, "how largely the thoughts and the behavior of individuals are determined by factors arising not from the inborn characteristics of the thinker or actor but from the cultural conditions affecting all the members of his group jointly" (79, p. 277; Bartlett, 7, 8). Lowie's chapter on association is one full of stimulating suggestions and problems, and illustrates how necessary it is, when studying human behavior, to take into consideration the cultural elements.

The actual ways in which culture is disseminated from one people to another is interesting, and has attracted the attention of Wissler (146). It has been found, for example, that certain unrelated complexes (for example, pottery and agriculture in North America, and cattle owning and patrilineal social organization in Africa) are almost always found together. Such a union of functionally nonrelated traits is called an adhesion, and is claimed to arise from the fact there was an approximately simultaneous invention at a culture center with the result that the two complexes started their spread together. Here we may have an historical factor accounting for the development and spread of important phases of culture, and such historical factors play a large part in the theory of the students of culture (134). Hobhouse's study of material cultures of certain simpler peoples illustrates the significance to psychology and sociology of the study of adhesions (58).

Before 1919, Nelson (94), working among the potsherds of the pueblos of the southwest, came to the conclusion that the diffusion had taken place from a center and that this distribution was concentric. This being the fact, he argued, the archaic types might be

found at the periphery of the circle, while, since the center of diffusion would have had the technique for the longest time, it would be located at the spot at which might be found the greatest technical development in pottery. This theory has been applied to cultural data by Kroeber (**66**) and Wissler (**145**). Recently Wissler has returned to this thesis and has developed it with much elaboration, as applied not only to cultural data but somatic, as well (**146**).

Diffusion of traits in this concentric form raises the question of *time,* and Wissler assumes (**139, 145**) that there is a relation between the distance a trait has spread and its age. That there are grave dangers in establishing the time factor is admitted, and Wissler points out that "the principle of agreement between age and distance from the center of origin, though true in general, works best when comparisons are made within the same trait-complex." The principle of diffusion as a criterion of age has been seriously questioned by Wallis (**130, 132**), who argues that comparative distribution is not necessarily evidence of comparative age, and moreover suggests that the simile used by Wissler and Kroeber, that of traits spreading like ripples when a pebble is dropped in a pond, is erroneous. Rather he argues that the spread of traits is to be compared to a stream of water seeking its own level. The point is of some importance, for if remoteness from the center is a factor in diffusion, there is then basis for interpreting the backwardness of peripheral peoples on a cultural basis: they are backward because culture traits have not spread to them from the cultural centers.

Wissler has also raised the point concerning the reasons for the form of trait distribution (**145, 146**), and the nature of the factors that influence the spread. His is an ecological explanation, and his most recent work holds suggestions that give much promise of aiding in the interpretation of cultural phenomena, although the limiting, rather than the determining influence of environment in shaping culture has been stressed by most of the American anthropologists at one time or another. The relation of ecological areas to culture has also been suggested by an Oriental sociologist, Mukerjee (**92**). The significance of the culture *center*—the "focal point"—has been shown by Kroeber (**66**), who also stresses the fact that a broad interpretation of history must take into consideration the shift of focal centers over a period of time, and the significance of this.

The consideration of diffusions leads naturally to another moot problem in anthropology which centers in the question: How does any group acquire its culture? This may be put in a slightly different way: If similarities of traits are seen among two more or less

distinct peoples, how is such similarity to be explained? Have we here independent inventions, evidence of cultural contacts, or an example of what the anthropologists call convergence? (Boas, **16**; Goldenweiser, **36**; Kroeber, **66**; Lowie, **72**; Rivers, **107**; Wallis, **128**; Wissler, **145**). The problem of diffusion as it bears upon these questions has been given much attention by the American school. While fully appreciating the significance of diffusion, they insist that there not be a blanket explanation in terms of it.

That identities between cultures far removed exist, no one would deny; the data show these similarities clearly. The problem lies in interpreting these similarities (Rivers, **107**, p. 61). To the older evolutionary school of ethnologists these similarities were the first to count, and were offered as proof of the psychic unity of mankind and the similar development of culture the world over. However, as data accumulated, this comparative method, as it was called, no longer seemed valid, for, although a phenomenon may be called by the same name by investigators in two different places, it does not follow at all that there is actual similarity in the minds of the peoples in whose culture the trait is found. This older method retains its popularity, as is seen by the ever-recurring editions of Frazer's *Golden Bough,* Westermarck's *History of Human Marriage,* and the recently republished work of Crawley on primitive sex life, *The Mystic Rose.*

It was such uncritical use of data which seemed to be identical that provoked, for example, the numerous discussions of totemism, which, though associated with the practice of claiming descent from, and taboos with regard to, certain animals, has been shown by Goldenweiser (**29**) to be psychologically different in different parts of the earth. Yet the method of comparing cultural phenomena, even of the most intangible types, persists in spite of the critical analyses of the American anthropologists. The psychologists and sociologists fall into this error constantly. It is true that the extreme evolutionary position, which claims a psychic unity of mankind as manifested in the independent invention of all cultural facts by all peoples, is not held to-day, but the comparative method is being applied by the *kulturhistorische Schule* of Graebner and his group in Germany. The work of Graebner (**42**, **43a**) represents the systematization of the beliefs with regard to this problem. The school is often called "the diffusionist school," for its members believe, as has been said, that cultural phenomena have been invented only once. Their work has thus far been almost entirely in the field of material culture, which lends itself to such technique and interpretation. For, laid

away in the museum, the paddle from South America and the paddle from Africa will not seem too dissimilar. And when a number of these cultural traits are seen to be the same, the criteria of form and quantity may be applied, and then historical connection may be posited. The *Ferninterpretation* principle, by means of which distance may be disregarded, completes the logical chain, and with these tools the entire mapping of historically connected cultures may proceed to the satisfaction of this school. Naturally such a view, which is so much of the museum and so little of the life of the people who are being studied, was vigorously attacked by the American students. Lowie has felt the disregard of the psychological factors involved—the attitude of the people towards a trait. The attitude or "set" may be different in two groups although the trait, objectively, may seem identical; in the life of the two groups the seemingly similar traits are subjectively dissimilar. Says Lowie (72): ". . . where the principle of psychic unity cannot be applied, and where the paths of diffusion cannot be definitely indicated, we must first inquire whether the supposed identities are really such, or become such only by abstracting from the psychological context in which they occur, and which determines them—whether, that is to say, we are comparing cultural realities, or merely fragments of our logical modes of classification." Boas (16), Goldenweiser (31, 35, 36), Sapir (109), and later Herskovits (56) and others have commented upon the principles of this school, and the implications of the doctrine. These Americans have stressed the position that the neglect of the psychological element in the spread and implications of cultural facts, and the disregard of actual historical relations between cultures assumed to be historically connected upon the basis of rigid criteria applied to material traits, does not make for a method of attack on this problem of diffusion *vs.* independent development, or theories regarding the spread of cultures, which can be regarded as tenable.

This problem has also claimed the attention of an English school headed by G. Elliot Smith (115, 116), and one of whose principal exponents is W. E. Perry (100). This group eventually received the endorsement of Rivers. The idea of diffusion was probably taken by this group from the German school, and the comparative method comes naturally, since it is traditional to English thinkers from the time of the great evolutionary ethnologists. In the hands of this English group the principle of diffusion and the comparative method have been combined to produce an almost perfect *reductio ad absurdum*. To them the central diffusing point of all that can be called civilization is Egypt. Professor Smith, an anatomist who

worked on Egyptian mummies, saw what to him were aspects of Egyptian culture all over the world, and plunged into the problems related to this discovery, with the results stated in the book already cited.

Here again, as with the Germans, there seems to be almost a total lack of understanding of the psychological mechanisms involved in the transmission of cultural traits, and the actual method whereby the diffusion of the culture traits considered by these men is studied is the same erroneous comparison of traits without regard to their place in the minds of those who employ them as cultural devices. From Egypt "civilization," as they reconstruct it, went in a main current through India, jumped the Pacific by way of Melanesia and Polynesia, and finally was planted on the shores of America, to flower in Mexico and Peru. Its spread in the other direction, into the British Isles, has recently been formulated by Massingham, another of this school (**88**). Here again, the mere fact that there are monoliths, that gold and pearls are sought after, that structures which may, by stretch of the imagination, be termed pyramids are found, that mummification in one form or another is practised, are sufficient to prove unity of cultural origin to the members of this school. The phenomenon of the change in culture, which makes such a long term stability as the migration requires (particularly this stability in transmission) almost impossible, and the fact that in diffusion not an entire culture but parts of it congenial to the patterns already present are taken over by the accepting culture, are quite disregarded, while it is strenuously assumed and asserted that these "children of the sun," as the adventurers who brought the culture and their descendants are termed, were accepted as gods and their cultures taken over *in toto* by the folk whom they visited. The work of Perry (**100**) where he asserts that the Maya worked the gold mines of the southern Appalachians and the pearl beds of the Ohio River in prehistoric times, thus being the moundbuilders (pyramid makers), or the Mexican elephant hunting adventures of Professor Smith (**116**), are typical of the uncritical use of comparative material of which they are guilty, and illustrate the extremes to which they will go in their diffusionist position.

As against this, the American anthropologists have attempted to study definite diffusions of definite cultural traits, going into the manner in which the diffusion took place, the number of salient points, both of the trait and the psychological setting in which it is found, and the ways in which it might have been diffused. For example, Boas (**20**) tells us that "a purely inductive study of ethnic

phenomena leads to the conclusion that mixed cultural types that are geographically or historically intermediate between two extremes, give evidence of diffusion," and that when two fundamentally different types of a social phenomenon come into contact the result may be a creation of a third intermediate type due to a process of what may be called double diffusion. (See Bartlett, 7.) Whatever may be the exact process in one locality or another, it seems quite obvious that both borrowing (or diffusion) and independent invention are at work in the production of new cultural traits. To answer the original question, How does a group get its culture? both of these processes must be considered.

Certainly the material cited thus far, and monographic studies of various peoples in different parts of the earth, indicate that the culture of any group undergoes change either through the influence of contact with external groups, or because of inventions or changes which are made within the group, or both. These changes caused by invention Boas sees arising through the working of what he has called the inner forces of society (**19**). Just how such changes operate is not altogether understood, and thorough study of them will involve consideration of the psychology of invention, some important phases of which have already been suggested by Goldenweiser (**36**, pp. 157–164), and Wissler (**145**). Studies, too, such as that of Mauss (**89**), are of interest in that they suggest possible psychological attitudes which may make for change from within a culture. This problem of the relation of the individual to his culture, and his influence upon it, is discussed by Lowie (**79**), who produces much evidence to show that "the individual is not *completely* merged in his social milieu—he reacts to it *as* an individual, that is, differently from every other group member. The cultural tradition of his people dominates him, but it is reflected in a distinctive fashion by each psyche. Were it otherwise, novel conceptions would never arise" (**79**, p. 221). The reciprocal action of culture and individual is illuminatingly treated by Judd (**59a**). Here, then, in this individual variability lies the cue to those inner forces of which Boas speaks (**19**). It is this same type of analysis that Bartlett has contributed (**7**). Radin's study of the autobiography of a Winnebago Indian (**103**) further serves to illustrate how the individual, in primitive culture as in our own, does respond to his cultural environment, and how his unique responses can set in motion these subtle changes that make for cultural instability. Malinowski (**84**) and Michelson (**90a**) have contributed further important data on this point. As Boas points out (**19**), these inner changes that are constantly taking place, as well as the adaptation of

differing culture traits to the existing pattern, make primitive culture much less stable than has frequently been alleged. The complexity of life to which the primitive man must adjust himself—become conditioned—is made strikingly apparent from a consideration of the monographic literature. Moreover it becomes increasingly evident that psychologically life in primitive society is as full and as complete as it is in any modern society (Boas, **10**, **18**; Goldenweiser, **36**; Lowie, 77; Sapir, 111). This influence upon the mind of the individual of the cultural milieu has been especially stressed by Ogburn and Kroeber (**62**, **98**), who, while they recognize the importance of the individual as the creative influence, nevertheless see him conditioned in his creativeness by the existing development of the culture in which he is living. By these cultural determinists a constant supply of inventive ability is assumed, which inventive ability reacts upon the cultural base of the moment. Thus, to follow Ogburn, necessity is not to be conceived so much the mother of invention as is the size of the existing culture base (**98**). The fact of multiple independent invention is cited by both Ogburn and Kroeber in seeking to establish this point (Ogburn, **97**, **98**, **99**; Kroeber, **62**). Once more, the importance of this concept for psychology is discernible: "backward" peoples may be those whose culture base is but little developed, and in consequence of which change within the group would be slow.

Ogburn (**98**) has also developed another concept which is suggested by the cumulative character of culture. Not all phases of culture change at the same rate. In any group, however, the various phases of culture must be adjusted to each other. This would follow from the pattern concept.[3] The period between a change in one phase of culture and the readaptation of the other phases, Ogburn designates as the period of *social lag*. Social lag arises through the failure of one phase of culture to adjust itself promptly to changes in other phases of the culture with which it is related. To the sociologist this concept of social lag is of vast importance, for it presents a new method of approach to social problems, most of which can be analyzed as examples of modern lag (**135**). This also suggests a slightly different concept of *conservatism:* a conservative individual is one who is conditioned to a given set of cultural patterns,

[3] Ogburn does make a distinction between what he calls adaptive and non-adaptive culture—adaptive culture being that part of the total culture of a group which must be realigned to coincide with other cultural changes; non-adaptive culture being that part of the total which does not require readaptation: such as art, literature, etc. See *Social Change*, Part IV.

and who resists the reconditioning necessary to bring conformity to the changing culture of the group.

IV. *The Study of Culture.* Fully to appreciate the changes that have taken place in the last decade or two in the methods employed in studying culture, one must revert to anthropological discussions as formulated by the evolutionists. Herbert Spencer and Tylor (126) may be considered as the fathers of the evolutionary hypothesis. As has been pointed out, the evolutionary interpretation is built upon the assumption of the unity of mind, so that under the influence of generally similar physical environment man's mind works in similar manner and produces similar culture (Boas, 12, 19; Goldenweiser, 35, 36, 39). Not only this, but having started at similar stages of culture, subsequent cultural development will in all cases proceed through similar stages. The changes that take place in this development, moreover, will be ever uniform, gradual, and progressive. The evolutionary theory, with its stage notion of development, resulted from the application in an unwarranted manner of the comparative method, but erroneous though the results may have been, they have exercised a tremendous grip upon social theory generally. While Spencer developed the theory in general, it was applied in specific instances by numerous writers who sought to establish the sequences through which the several phases of a total culture had passed, as in the work of Frazer (28), Balfour (4), Haddon (45), Morgan (91), and numerous others. (See Goldenweiser, 39.) In Germany, Wundt (148) was at the same time escaping many of the more flagrant pitfalls of the more extreme evolutionists.

How this evolutionary position was gradually broken down is summarized by Goldenweiser (35, 39, 40); it is needless here to say more than that the older position has been demolished under the vast collection of data resulting from intensive study of the cultures of limited areas. In place of the comparative method there was developed the historical method in the study of culture, and this has perhaps been the chief contribution of the American anthropologists. The Americans have not been alone in this swing from the evolutionary position for the whole extreme diffusionist position of the German and English groups discussed above may be considered as a reaction.

The discrediting of the evolutionary theory was accompanied also by a more careful examination of other interpretations of social development. The American anthropologists have paid considerable attention to the theories of both the geographical and racial determinists.

The geographical determinists were perhaps easiest to dispose of. Boas (18), Goldenweiser (32, 36), Kroeber (66), Lowie (76), Ogburn (98), Wallis (131, 132), and Wissler (137, 143) have all shown that while there is obviously a relation between the physical environment and culture, the relation is a limiting rather than a causative one. Nature offers man, in the way of raw material, certain possibilities for cultural development, but a study of the raw material alone will give no clue concerning the culture that will be built from it. This can be understood only in the light of historical factors. One finds, as these students have pointed out, peoples living in almost identical environments with diverse cultures, and other peoples with cultural similarities in unlike environments (76).

The problems of culture and biology fall into two groups, those centering specifically in the subject of race and race differences, and those bearing on the extent to which cultural changes are correlated with biological change. Ogburn (98) has given particular attention to the latter, coming to the conclusion that available evidence fails to show that there is a correlation between culture and biological change; the great cultural changes have taken place faster than it would be possible for mutations to occur which would underlie biological changes. Thus, to him, the biological factor in cultural development may be considered as a constant, and all explanations of change in culture sought for in culture itself. This position has been criticized by Hankins most recently (48), who argues that vast changes may take place in a given population, if not by mutation, then at least through social selection and crossings, though he has not offered proof that these biological changes have cultural changes causally related to them.

The importance of the investigation into the existence or nonexistence of any relationship between culture and race has not escaped the members of the American anthropological group, although in the main its members are more occupied with the consideration of data which fall on the purely cultural plane. However, the fact cannot be overlooked that man is a biological animal as well as a culture building one, and the insistence upon a complete study of man as represented in both the aspects is generally recognized. This is particularly true of the work of Boas (12, 18, 22, and numerous other papers), who insists that this unity of the entire content of anthropology be preserved. His position, which is essentially that of those students of his who have touched upon the subject, is this: that as far as the relation of innate ability to the state of culture which a given people may have produced is concerned, the

historical contacts of any given culture, and the factor of accidental discovery of cultural traits bulk so large, that the innate culture making ability is relatively unimportant. (See Goldenweiser, **36**, **37**, **38**; Herskovits, **49**, **52**, **53**; Kroeber, **66**; Lowie, **78**; Ogburn, **98**; Tozzer, **125**; Wallis, **133**.) While it is not true that these men have asserted equality in ability between racial groups, they question the possible assertion to the contrary, *i.e.*, that there are inferior races, since, as Boas has pointed out (**18**), and as his students have often remarked, the differences within racial groups are so much greater than the differences between any two races that these latter differences are largely overshadowed. Hankins (**48**) vigorously presses the point of the differing numbers of individuals who are to be found at the upper end of intelligence distributions for the several races, but his willing acceptance of the results of the intelligence tests, added to his argument upon the purely biological plane rather than the cultural, leaves his conclusions on the subject still open to question. Certainly, if we consider the cultures erected by the groups included within any racial grouping, there is such a vast variety in them, both as to complexity and organization, that correlation of race with culture is exceedingly precarious. There must be available far more exact definition of the term " race " than at present exists, and more complete investigation into the processes of cultural development, before such correlations can be attempted. And while cultural facts can exert selective influences which will shape the physical form of the people (Herskovits, **54**, **55**), there have, to our knowledge, been produced to date no data which will stand under critical survey and which show that physical form influences culture. Boas has shown how independently race, language, and culture can operate, and how unrelated their distributions are found to be (**15**), and until our knowledge of their interrelation is far greater, further analysis into the several fields represented is greatly to be desired. As far as the cultural aspect is concerned, it would seem that before attempts to explain cultural differences on the basis of race are made, wiser methodology would suggest that an explanation in terms of culture itself should be undertaken.

This same group of writers has also pointed out the limitations of a strict psychological interpretation of culture. (See especially Boas, **18**; Lowie, **76**.) That there must be a consideration of psychological factors in cultural interpretation is clear to most of the group; in fact, Goldenweiser (**35**, **39**, **40**) makes this apparent when he characterizes, in a summary article, the American anthropologists as " critical, historical, psychological." While there has been objection

raised to the type of psychological interpretation illustrated in Freud's *Totem and Taboo* (Kroeber, 65, 66; Wissler, 136) and works of a similar nature (such as Roheim's *Social Anthropology*), wherein there is a generalized psychological interpretation of culture (108, 147), the culturalists have long been aware that findings of psychology must be employed in interpreting their objective cultural data (*e.g.*, Boas, 14). While arguing against the generalized psychological interpretation of society, or generalization regarding the psychology of primitives, such as that of Levy-Bruhl (69, 70), the relation of culture to psychology, as it bears upon the problem of method, has been raised by Boas (14, 18), Ellwood (26), Goldenweiser (34), and others. At the same time it is pointed out by these men that there are phases of culture that need more interpretation than can be given by psychology alone.

Recognition of this latter fact has led some of the cultural group to an extreme position in cultural determinism, as in the case of Ogburn (98, 99) and Kroeber (61, 62, 63), who objectify culture more completely than any of the other students in the field. The individual is a secondary factor to these writers, which has led to strict criticism of them, in this respect, by Goldenweiser (33, 39), Sapir (110), and Haeberlin (46). W. I. Thomas has also stressed the importance of the individual; he was at the same time one of the first of the sociologists to see the significance of the anthropological approach (122, 123).

In recent times, so recent, indeed, that it is difficult to find studies which give the theoretical position of the group, there has arisen the "functional" school, which has been mentioned above. This development came in part as a reaction, particularly by Malinowski, to the plotting of diffusions by the Graebnerian and the Elliot Smith schools, and in part because of a lack of understanding of the interest which the American school has in the interrelation of the aspects of cultural elements within the cultures studied, and the psychology of the individuals who are being studied. By the functionalists there is a sharp insistence on this latter aspect of the matter. Thus, Malinowski (81) states the position of this group: "This type of theory aims at the explanation of anthropological facts at all levels of development by their function, by the part which they play within the integral system of culture, by the manner in which they are related to each other within the system, and by the manner in which the system is related to the physical environment. It aims at the understanding of the nature of culture, rather than at conjectural reconstructions of its evolution or past historical events." It is clear that Malinowski at

least agrees with the field work methods of the Americans (**84**) in his insistence on the collection of textual data instead of the giving of impressions and generalizations about the cultures being studied, which is so characteristic of reports of work carried on by some of the British school. But he does not agree with either the study of distributions or the historical method which the American group have developed. Thus, he charges that ". . . most of the American anthropologists have failed to disentangle the empirical interpretation of culture in terms of function from reconstruction in terms of conjectural history" (**81**, p. 133). He pleads (**86**) for a study of the interrelation of traits within the tribe, rather than for the ethnologist to waste his time making these historical reconstructions. His field work is excellent (*e.g.*, **82**), but it is obvious from our discussion in this paper that his tendency to generalize for all humanity from his work with the Trobriand Islanders (**82**, **84**, **85**) is methodologically as dangerous as are the dogmatizations of the evolutionary or diffusionist schools which he excoriates.

Paul Radin, on the other hand, while accepting the functionalist position, is rather insistent on the psychological aspects of primitive culture, and the reaction of the individual to it, and, as opposed to the cultural deterministic position, the manner in which an individual can be an influence for cultural change. His discussion of the spread of the peyote cult among the woodland Indians (**102**), and his autobiography of a Winnebago Indian (**103**), illustrate this position adequately, and his later work (**105**) extends this interest. In his editorial foreword to the journal which he edits, *Primitive Culture* (**104**), he states his position as one of insistence on the human value of primitive life. While the collection of archive material is to be urged, yet he feels that the antiquarian interest which this represents is highly undesirable. Therefore, he says, his journal is to "stress just this *functional, dynamic,* and *human* side of primitive culture. . . . It is not our purpose to . . . deal with the distributions of cultures or cultural elements." And the interest in the psychological side, of course, should be carried over to the important and almost untouched field of a consideration of the effects on primitive cultures of contact with white civilization, and the processes of adjustment—a field touched in part by Bartlett (7) and Rivers (**106**, **107**).

Out of all the work of the American anthropologists, work which, as Boas states (**19**), has been so detailed as to lead to some criticism that more ultimate generalizations were being lost sight of, has come the firm insistence upon the historical point of view in the explanation

of cultural phenomena. In fact, the insistence upon the explanation of social phenomena in terms strictly historical may be looked upon as the chief characteristic of the method of approach of this American group. It sets them off alike from the early evolutionists and the extreme diffusionists (19). The basic contention is that each culture group must be investigated and analyzed in the light of its own historical development and contacts, with consideration also given to the process of psychological association that takes place within the group, as well as attention to other factors leading to inner changes that are important in understanding the culture of the given people being studied (Boas, 16, 18; Goldenweiser, 34, 39, 40; Kroeber, 66; Lowie, 79; Ogburn, 96). Boas (19, p. 317) states the method in brief as follows: "Each cultural group has its own unique history, dependent partly upon the peculiar inner development of the social group and partly upon the foreign influence to which it has been subjected." Goldenweiser (35, p. 63) has phrased it this way: "On the one hand primitive cultures are examined in the totality of their present interrelations, each tribe being considered both as a unit and in its relations to other tribes. On the other hand cultural changes which are to be interpreted historically are referred to cultural antecedents, not to racial, environmental, or general psychological ones." The significance of this method for modern sociology has been seen by Ogburn, especially in those problems calling for a separation of psychological and cultural factors. The first step, Ogburn argues (96), is to determine the cultural factor. "If the attempt is made to determine the psychological factor before the cultural factor is known, the probability of error is generally so great as to make it untrustworthy."

Like so many other aspects of the thought of this school, the methodological position comes more by implication from the work its members have accomplished than from actual summary statement. This follows from the preoccupation with details of cultural data characteristic of their work, yet, fortunately, Sapir has given us an important extended methodological statement (109) which may be taken, in the main, as representative of the actual methodology of the American anthropologists. Asserting at the outset that ethnology is an historical science, he then proceeds to discuss the types of evidence which may be gathered so as to ferret out the historical development of the cultures of nonhistorical peoples. He first deals with direct testimony, that of early records of travelers, traders, and the like, the testimony of natives as given in myths, and the stratigraphic evidence of the passage of time offered by archaeological excavation. He then passes to inferential testimony which obviously must make

up the greatest portion of what is gathered by the historical ethnologist in his researches. First, we have the data of physical anthropology, which may give a cue through the detection of resemblances in physical form of the descendants of peoples once in contact who to-day are far apart. Then, ethnological data, where there are considered in turn the likelihood of seriation from simplicity to complexity as an indication of the passage of time, and cultural associations which, whether "organic (logically intelligible) or of a purely fortuitous character," constitute a "powerful method for the determination of the relative ages of cultural elements" (p. 15). Next Sapir considers the phenomenon of diffusion, and while cautioning against the uncritical use of criteria such as characterize the Graebnerian school, shows how intensive study of the spread of a trait or series of traits may give further insight into the historical development of the trait in a given culture. He then passes to the culture area as a tool, which he considers more as a classificatory device and an aid to the psychological interpretation of cultural phenomena as set forth by the American school than as an aid to historical dissection. Finally, there is a detailed consideration of the use of linguistic evidence, such as place names and culture words. The method, of course, as presented by him, is oriented about its usefulness in dissecting American data, but there is no reason why the principles enunciated in it cannot be utilized profitably in any region. And in his concluding paragraphs we feel the restraint which characterizes the productions of the American group, particularly the insistence on the use of only such parts of the method as are applicable in any given case. Finally, he stresses recognition of "the danger of tearing a culture element loose from its psychological and geographical (*i.e.*, distributional) setting."

That a valid understanding of the development and significance of culture in the life of human beings stands thus largely on a conception of the interplay between the psyche of the individual in a culture and the culture of which he is a part must be evident. His behavior during his life is determined by the character of the culture into which he is born, but the extent to which, during his lifetime, he may influence this culture forms a vast field for study as yet almost untouched. Both sides of the problem must be examined further; there is great need for knowledge for the processes of culture no less than for the psychology of the individuals who use culture as a tool in the business of living. And to such an understanding it is principally the psychologists and the anthropologists who can make the most telling contributions.

REFERENCES

1. ALLPORT, F. H., Social Change: An Analysis of Professor Ogburn's Culture Theory. *J. of Social Forces,* 1924, 2, 671–676.
2. ALLPORT, F. H., The Present Status of Social Psychology. *J. Abnorm. and Soc. Psychol.,* 1927, 21, 372–383.
3. ANKERMANN, B., Kulturkreise und Kulturschichten in Afrika. *Zeits. f. Ethnol.,* 1905, 37, 54–84.
4. BALFOUR, H., *The Evolution of Decorative Art.* London: Rivington, Percival, 1893. Pp. xv+131.
5. BARNES, H. E. (editor), *The History and Prospects of the Social Sciences.* N. Y.: Knopf, 1925. Pp. xxi+534.
6. BARNES, H. E., *The New History and the Social Studies.* N. Y.: Century, 1925. Pp. xvii+605.
7. BARTLETT, F. C., *Psychology and Primitive Culture.* N. Y.: Macmillan, 1923. Pp. ix+294.
8. BARTLETT, F. C., Group Organization and Social Behavior. *International J. of Ethics,* 1925, 35, 346–367.
9. BENEDICT, R. F., The Concept of the Guardian Spirit in North America. *Memoir 29, American Anthropological Assn.,* 1923. Pp. 97.
10. BOAS, F., The Central Eskimo. *Sixth Annual Report, Bur. of Amer. Ethnol.,* 1884–1885, 409–667.
11. BOAS, F., The Social Organization and Secret Societies of the Kwakiutl Indians. *Annual Report of the United States National Museum,* 1895, 311–738.
12. BOAS, F., *Anthropology.* N. Y.: Columbia Univ. Press, 1908. Pp. 28.
13. BOAS, F., Tsimshian Mythology. *Thirty-first Annual Report, Bur. of Amer. Ethnol.,* 1909–1910, 31–1037.
14. BOAS, F., Psychological Problems in Anthropology. *Amer. J. of Psychol.,* 1910, 21, 371–384.
15. BOAS, F., *Handbook of American Indian Languages,* 2 vols. (Introduction; Vol. I, pp. 5–74). Washington: Bureau of American Ethnology, 1911.
16. BOAS, F., Review of Graebner's *Methode der Ethnologie. Science* (n. s.), 1911, 34, 804–810.
17. BOAS, F., Ethnology of the Kwakiutl. *Thirty-fifth Annual Report, Bur. of Amer. Ethnol.,* 2 volumes, 1913–1914, 41–1475.
18. BOAS, F., *The Mind of Primitive Man.* N. Y.: Macmillan, 1913. Pp. x+294.
19. BOAS, F., Methods of Ethnology. *Amer. Anthrop.* (n. s.), 1920, 22, 311–321.
20. BOAS, F., Evolution or Diffusion? *Amer. Anthrop.* (n. s.), 1924, 26, 340–344.
22. BOAS, F., Fallacies of Racial Inferiority. *Current History,* 1927, 25, 677–682.
23. BOGARAS, W., The Chuckchee. *Publications of the Jessup North Pacific Expedition (American Museum of Natural History),* Volume 7, 1909.
24. BRIGHAM, C. C., *A Study of American Intelligence.* Princeton: Princeton Univ. Press, 1923. Pp. xxv+210.
25. CLARK, W. W., Los Angeles Negro Children. *Educ. Res. Bull., Los Angeles City Schools, III,* 1923, 1–2.

26. ELLWOOD, C. A., Theories of Cultural Evolution. *Amer. J. Soc.*, 1918, 23, 779–800.

27. ELLWOOD, C. A., The Development of Sociology in the United States. *Sociol. Rev.*, 1927, 19, 25–34.

28. FRAZER, J. G., *The Golden Bough.* London: Macmillan, 1894, 2 vols.

29. GOLDENWEISER, A. A., Totemism: An Analytical Study. *J. Amer. Folk-Lore*, 1910, 23, 179–293.

30. GOLDENWEISER, A. A., The Principle of Limited Possibilities in the Development of Culture. *J. Amer. Folk-Lore*, 1913, 26, 259–290.

31. GOLDENWEISER, A. A., The Social Organization of the Indians of North America. *J. Amer. Folk-Lore*, 1914, 27, 411–436.

32. GOLDENWEISER, A. A., Culture and Environment. *Amer. J. Soc.*, 1916, 21, 628–633.

33. GOLDENWEISER, A. A., The Autonomy of the Social. *Amer. Anthrop.* (n. s.), 1917, 19, 447–449.

34. GOLDENWEISER, A. A., History, Psychology and Culture: A Set of Categories for an Introduction to Social Science. *J. Philos., Psychol., and Sci. Meth.*, 1918, 15, 561–571, 589–607.

35. GOLDENWEISER, A. A., Four Phases of Anthropological Thought. *Pub. Amer. Sociological Soc.*, 1921, 16, 50–69.

36. GOLDENWEISER, A. A., *Early Civilization.* N. Y.: Knopf, 1922. Pp. xiv+428.

37. GOLDENWEISER, A. A., Racial Theory and the Negro. *Opportunity*, 1923, 1, 229–231.

38. GOLDENWEISER, A. A., Race and Culture in the Modern World. *J. Social Forces*, 1924, 3, 127–136.

39. GOLDENWEISER, A. A., Cultural Anthropology. A chapter in *The History and Prospects of the Social Sciences* (H. E. Barnes, editor). N. Y.: Knopf, 1925.

40. GOLDENWEISER, A. A., Diffusionism and the American School of Historical Ethnology. *Amer. J. Soc.*, 1925, 31, 19–38.

41. GRAEBNER, F., Kulturkreise und Kulturschichten in Ozeania. *Zeit. f. Ethnol.*, 1906, 38, 28–53.

42. GRAEBNER, F., *Methode der Ethnologie.* Heidelberg: Winter, 1911.

43. GRAEBNER, F., Die Melanesische Bogenkultur und ihre Verwandten. *Anthropos*, 1909, 4, 726–780, 998–1032.

43a. GRAEBNER, F., Ethnologie, pp. 435–572, in *Anthropologie*, edited by G. v. Schwalbe and E. Fischer. Teil 3, Ab. 5 of *Die Kultur der Gegenwart.* Leipzig and Berlin: Teubner, 1923.

44. GRANT, M., *The Passing of the Great Race.* N. Y.: Scribners, 1916. Pp. xxi+245.

45. HADDON, A. C., *Evolution in Art.* London: W. Scott, Ltd., 1895. Pp. xviii+364.

46. HAEBERLIN, H. K., Anti-Professions. *Amer. Anthrop.* (n. s.), 1915, 17, 756–759.

47. HALLOWELL, A. I., Bear Ceremonialism in the Northern Hemisphere. *Amer. Anthrop.* (n. s.), 1926, 18, 1–175.

48. HANKINS, F. H., *The Racial Basis of Civilization.* N. Y.: Knopf, 1926. Pp. x+384.

49. HERSKOVITS, M. J., What Is A Race? *Amer. Mercury*, 1924, 2, 207–210.

50. Herskovits, M. J., A Preliminary Consideration of the Culture Areas of Africa. *Amer. Anthrop.* (n. s.), 1924, **26**, 50–63.
51. Herskovits, M. J., Social Pattern: A Methodological Study. *Social Forces*, 1925, **4**, 57–69.
52. Herskovits, M. J., The Dilemma of Social Pattern. *Survey*, 1925, **6**, 676–678. Reprinted in *The New Negro* (Alain Locke, editor). N. Y.: Boni, 1925.
53. Herskovits, M. J., Brains and the Immigrant. *Nation*, 1925, **70**, 139–141.
54. Herskovits, M. J., Social Selection in a Mixed Population. *Proc. Nat. Acad. of Sci.*, 1926, **12**, 587–593.
55. Herskovits, M. J., Some Effects of Social Selection on the American Negro. *Pub. Amer. Sociological Soc.*, 1926, **32**, 77–82.
56. Herskovits, M. J., The Cattle Complex in East Africa. *Amer. Anthrop.* (n. s.), 1926, **28**, 230–272, 361–388, 494–528, 633–664.
57. Herskovits, M. J., On the Relation Between Negro-White Mixture and Standing in Intelligence Tests. *Ped. Sem. and J. Gen. Psy.*, 1926, **33**, 30–42.
58. Hobhouse, L. T., Wheeler, G. C., and Ginsberg, M., *The Material Culture and Social Institutions of the Simpler Peoples*. London: Chapman, 1915. Pp. 299.
59. Josey, C. C., *Race and National Solidarity*. N. Y.: Scribners, 1923. Pp. ix+227.
59a. Judd, C. H., *The Psychology of Social Institutions*. N. Y.: Macmillan, 1926. Pp. x+346.
60. Kroeber, A. L., Decorative Symbolism of the Arapaho. *Amer. Anthrop.* (n. s.), 1901, **3**, 308–336.
61. Kroeber, A. L., Eighteen Professions. *Amer. Anthrop.* (n. s.), 1915, **17**, 285–288.
62. Kroeber, A. L., The Superorganic. *Amer. Anthrop.* (n. s.), 1917, **19**, 163–213.
63. Kroeber, A. L., On the Principle of Order in Civilization as Exemplified in Changes of Fashion. *Amer. Anthrop.* (n. s.), 1919, **21**, 235–263.
64. Kroeber, A. L., California Culture Provinces. *Univ. of California Pub. in Amer. Arch. and Ethnol.*, 1920, **17**, 151–169.
65. Kroeber, A. L., Totem and Taboo: An Ethnological Psychoanalysis. *Amer. Anthrop.* (n. s.), 1920, **22**, 48–55.
66. Kroeber, A. L., *Anthropology*. N. Y.: Harcourt, 1923. Pp. x+523.
67. Kroeber, A. L. *Handbook of the Indians of California*. *Bull.* **78**, Bur. of Amer. Ethnol., Washington, 1925.
68. Lang, A., *Custom and Myth*. N. Y.: Harpers, 1885. Pp. 312.
69. Levy-Bruhl, L., *Primitive Mentality*. N. Y.: Macmillan, 1923. Pp. 458.
70. Levy-Bruhl, L., *How Natives Think*. N. Y.: Knopf, 1926. Pp. 392.
71. Long, H. H., On Mental Tests and Racial Psychology: A Critique. *Opportunity*, 1925, **3**, 134–139.
72. Lowie, R. H., On the Principle of Convergence in Ethnology. *J. Amer. Folk-Lore*, 1912, **25**, 24–42.
73. Lowie, R. H., Some Problems in the Ethnology of the Crow and Village Indians. *Amer. Anthrop.* (n. s.), 1912, **14**, 60–71.
74. Lowie, R. H., Ceremonialism in North America. *Amer. Anthrop.* (n. s.), 1914, **16**, 602–629.

75. LOWIE, R. H., Psychology and Sociology. *Amer. J. Soc.*, 1915, **21**, 217–229.
76. LOWIE, R. H., *Culture and Ethnology*. N. Y.: Boni and Liveright, 1917. Pp. 189.
77. LOWIE, R. H., *Primitive Society*. N. Y.: Boni and Liveright, 1920. Pp. viii+463.
78. LOWIE, R. H., Psychology, Anthropology and Race. *Amer. Anthrop.* (n. s.), 1923, **25**, 291–303.
79. LOWIE, R. H., *Primitive Religion*. N. Y.: Boni and Liveright, 1924. Pp. xix+346.
80. MCDOUGALL, W., *Is America Safe for Democracy?* N. Y.: Scribners, 1921. Pp. viii+218.
81. MALINOWSKI, B., Anthropology. Volume I of Supplementary Volumes of the XIIIth edition, *Encyclopedia Britannica*, 1926, 131–140.
82. MALINOWSKI, B., *The Argonauts of the Western Pacific*. London: Routledge, 1922. Pp. xxxi+527.
83. MALINOWSKI, B., *Myth in Primitive Society*. N. Y.: Norton, 1926. Pp. 94.
84. MALINOWSKI, B., *Crime and Custom in Savage Society*. N. Y.: Harcourt, 1926. Pp. 132.
85. MALINOWSKI, B., *The Father in Primitive Psychology*. N. Y.: Norton, 1927. Pp. 95.
86. MALINOWSKI, B., Useful and Useless Anthropology. *New Republic*, 1927, **50**, 109–111.
87. MARIN, L., *Questionnaire d'ethnographie: Table d'analyse en ethnographie*. Paris: Maisonneuve Frères, 1925. Pp. 129.
88. MASSINGHAM, H. J., *Downland Man*. London: Cape, 1926. Pp. 422.
89. MAUSS, M., Essai sur le Don, Forme archaique de l'écharge. *L'Année sociologique* (n. s.), 1923–1924, **30**, 186.
90. MEAD, M., The Methodology of Racial Testing: Its Significance for Sociology. *Amer. J. Soc.*, 1926, **31**, 657–667.
90a. MICHELSON, T., The Autobiography of a Fox Indian Woman. *40th Annual Report, Bur. of Amer. Ethnol.* (1918–1919), pp. 291–351.
91. MORGAN, L. H., *Ancient Society*. N. Y.: Holt, 1878. Pp. 560.
92. MUKERJEE, R., *Regional Sociology*. N. Y.: Century, 1926. Pp. x+287.
93. MURDOCK, K., and SULLIVAN, L., A Contribution to the Study of Mental and Physical Measurement in Normal Children. *Amer. Phys. Educ. Rev.*, 1923.
94. NELSON, N. C., Human Culture, Its Probable Place of Origin on the Earth, and Its Mode of Distribution. *Natural History*, 1919, **19**, 131–139.
95. NORDENSKIÖLD, E., *Comparative Ethnographic Studies* (especially volumes I, II, III). Gothenburg: 1919, 1920, 1924.
96. OGBURN, W. F., The Historical Method in Social Phenomena. *Pub. Amer. Sociological Soc.*, 1921, **16**, 70–83.
97. OGBURN, W. F., and THOMAS, D., Are Inventions Inevitable? *Pol. Sci. Quart.*, 1922, **37**, 83–98.
98. OGBURN, W. F., *Social Change*. N. Y.: Heubsch, 1922. Pp. viii+365.
99. OGBURN, W. F., The Great Man vs. Social Forces. *Social Forces*, 1926, **5**, 225–231.
100. PERRY, W. J., *Children of the Sun: A Study in the Early History of Civilization*. N. Y.: Dutton, 1923. Pp. xv+551.

101. RADCLIFFE-BROWN, A., The Methods of Ethnology. *South African J. of Sci.*, 1923.

102. RADIN, P., Sketch of the Peyote Cult of the Winnebago. *Amer. J. of Relig. Psychol.*, 1914, 7.

103. RADIN, P., *Crashing Thunder.* N. Y.: Appleton, 1926. Pp. xxv+202. Also published as *The Autobiography of a Winnebago Indian, Univ. of Cal. Pub. in Amer. Arch. and Eth.*, 1926, 7, 381–473.

104. RADIN, P., Editor's Foreword to Volume I, Number 1, of *Primitive Culture*, published at 39 Leopold Street, Mechelen, Belgium, 1926.

105. RADIN, P., *Primitive Man as Philosopher.* N. Y.: Appleton, 1927 (forthcoming).

106. RIVERS, W. H. R., *Essays on the Depopulation of Melanesia.* Cambridge: Cambridge University Press, 1922. Pp. xviii+116.

107. RIVERS, W. H. R., *Psychology and Ethnology.* N. Y.: Harcourt, 1926. Pp. xxviii+324.

108. ROHEIM, G., *Social Anthropology.* N. Y.: Boni and Liveright, 1926. Pp. 487.

109. SAPIR, E., Time Perspective in Aboriginal American Culture, A Study in Method. *Memoir 90 (No. 13, Anthro. Series.) Geological Survey of Canada*, Ottawa, 1916.

110. SAPIR, E., Do We Need a Superorganic? *Amer. Anthrop.* (n. s.), 1917, 19, 441–447.

111. SAPIR, E., Culture, Genuine and Spurious. *Amer. J. Soc.*, 1924, 29, 401–417.

112. SCHMIDT, P. W., Kulturkreise und Kulturschichten in Süd Amerika. *Zeit. f. Ethnol.*, 1913, 45, 1014*ff*.

113. SCHMIDT, P. W., *Die Sprachfamilien und Sprachenkreise der Erde.* (One volume and atlas.) Heidelberg: Winters, 1926.

114. SMITH, E. W., and DALE, A. M., *The Ila-Speaking Peoples of Northern Rhodesia.* London: Macmillan, 1920. 2 vols.

115. SMITH, G. E., *The Migrations of Early Culture.* Manchester: 1915. Pp. vii+143.

116. SMITH, G. E., *Elephants and Ethnologists.* London: Kegan Paul, French, Trubner and Co., 1924. Pp. viii+135.

117. SMITH, G. E., Anthropology. *Encyclopedia Brit.*, 12 ed., XXX, 147–155, 1922.

118. SPENCER, B., and GILLIN, F. J., *The Northern Tribes of Central Australia.* N. Y.: Macmillan, 1904. Pp. xxxv+784.

119. SPIER, L., The Sun Dance of the Plains Indians: Its Development and Diffusion. *Anthropological Papers, Amer. Museum Nat. Hist.*, 1921, 14.

120. STODDARD, T. L., *The Rising Tide of Color Against White World Supremacy.* N. Y.: Scribners, 1920. Pp. xxxii+320.

121. SUMNER, W. G., *Folkways.* Boston: Ginn, 1907. Pp. vii+692.

122. THOMAS, W. I., *Sourcebook for Social Origins.* Boston: Badger, 1909. Pp. xvi+932.

123. THOMAS, W. I., and ZANIECKI, F., *The Polish Peasant in Europe and America: Monograph of an Immigrant Group.* Chicago: Univ. of Chicago Press, 1918. 4 vols.

124. THRASHER, F. M., *The Gang.* Chicago: University of Chicago Press, 1927. Pp. xxi+571.

125. Tozzer, A. M., *Social Origins and Social Continuities.* N. Y.: Macmillan, 1925. Pp. xix+286.
126. Tylor, E. B., *Primitive Culture.* London: Murray, 1871. 2 vols.
127. Wallis, W. D., Individual Initiative and Social Compulsion. *Amer. Anthrop.* (n. s.), 1915, **17**, 647–665.
128. Wallis, W. D., Similarities in Culture. *Amer. Anthrop.* (n. s.), 1917, **19**, 41–54.
129. Wallis, W. D., The Independence of Social Psychology. *J. Abnorm. and Soc. Psychol.*, 1925, **20**, 147–150.
130. Wallis, W. D., Diffusion as a Criterion of Age. *Amer. Anthrop.* (n. s.), 1925, **27**, 91–99.
131. Wallis, W. D., Geographical Environment and Culture. *Social Forces*, 1926, **4**, 702–708.
132. Wallis, W. D., *An Introduction to Anthropology.* N. Y.: Harpers, 1926. Pp. xvi+520.
133. Wallis, W. D., Race and Culture. *Sci. Monthly*, 1926, **23**, 313–321.
134. Willey, M. M., and Herskovits, M. J., The Cultural Approach to Sociology. *Amer. J. Soc.*, 1923, **29**, 188–199. Reprinted in *Readings in Sociology* (Jerome Davis, editor). N. Y.: Heath, 1927.
135. Willey, M. M., Society and Its Cultural Heritage. In (Davis, Jerome, editor) *An Introduction to Sociology.* N. Y.: Heath, 1927.
136. Wissler, C., Anthropology. *Encly. Amer.*, 1925, **2**, 20–24.
137. Wissler, C., The Psychological Aspects of the Culture-Environment Relation. *Amer. Anthrop.* (n. s.), 1912, **14**, 217–225.
138. Wissler, C., Social Life of the Blackfoot Indians. *Anthro. Papers of the Amer. Museum of Nat. Hist.*, 1912.
139. Wissler, C., The Influence of the Horse in the Development of Plains Culture. *Amer. Anthrop.* (n. s.), 1914, **15**, 1–25.
140. Wissler, C., Material Culture of the North American Indians. *Amer. Anthrop.* (n. s.), 1914, **16**, 447–505.
141. Wissler, C., The Diffusion of Horse Culture Among the North American Indians. *Proceedings, Nat. Acad. Sci.*, 1915, **1**, 254–256.
142. Wissler, C., Aboriginal Maize Culture as a Typical Culture Complex. *Amer. J. Soc.*, 1916, **21**, 656–661.
143. Wissler, C., Psychological and Historical Interpretations for Culture. *Science* (n. s.), 1916, **43**, 193–201.
144. Wissler, C., *The American Indian.* N. Y.: Oxford University Press (second edition, 1922). Pp. xxi+572.
145. Wissler, C., *Man and Culture.* N. Y.: Crowell, 1923. Pp. xi+371.
146. Wissler, C., The Relation of Nature to Man in Aboriginal America. N. Y.: Oxford University Press, 1926. Pp. xx+248.
147. Wissler, C., Social Anthropology: A Review of Roheim's *Social Anthropology. Social Forces*, 1927, **5**, 519–520.
148. Wundt, W., *Elements of Folk Psychology.* N. Y.: Macmillan, 1916. Pp. xxiii+532.

CONCEPTS OF TRAIT AND PERSONALITY

BY GORDON W. ALLPORT
Dartmouth College

Bibliographies by May and Hartshorne (**25**), Manson (**23**), and Roback (**34**) bear out the statement of G. B. Watson (**42**) that "It is almost impossible to find a trait for which an adjective exists which has not been approached with some degree of suggestive investigation." Confusion has arisen from the fact that these investigations, with their ambition to determine magnitudes, give only an occasional glance at problems of terminology and theory.

On heuristic grounds it is, of course, possible to advocate measurement to the temporary neglect of theory. Symonds (**36**) believes that since we are never sure of the existence of traits nor of their definition, we should center attention upon reliabilities and let even validities await their turn. But the same writer has pointed out in another connection (**37**) that "attitudes" are defined in at least seven different ways by educators and psychologists. It is such confusion as this that retards the interpretation of reliabilities which Symonds is anxious to see established, and is generally prejudicial to advance in the field. Not only "attitude" but "disposition," "factor," and, above all, "trait" are employed vaguely to suit the needs of the moment. With such disingenuousness investigators have made it appear that quantitative distinctions can outrun qualitative, and that adjectival distinctions can outrun substantive. The truth of the matter is that neither measurement nor inventories of "attitudes" and the like can be intelligible until the substantives themselves are clearly understood.

The same term is frequently employed by a single writer to represent distinctly different levels of behavior. This method is found in its extreme form in Davenport's *Trait Book* (**8**), where tics, affability, and love of fishing are presented as parallel manifestations of personality. Marston (**24**) speaks of introversion and extroversion as emotional *traits,* and then lists twenty *traits* of introversion and twenty of extroversion. There is recognition here of at least two levels in behavior but both levels bear the same name. Ironically enough, Edman (**10**), after employing "trait" with almost the breadth of Davenport, remarks: "Social sciences will never attain the precision of the physical sciences until it also attains as clear and unambiguous a terminology."

There is likewise confusion between personality *devaluated* and personality *evaluated,* that is, between personality and character. To be sure, Watson (43), followed by many others, has marked the difference between the two. In practice it is a difficult but not an impossible distinction to maintain. The same behavior psychologically speaking may be moral in one locality, immoral in another, moral at one period of time, immoral at another. There are no "moral traits" until trends in personality are evaluated. Tests which deal with morality admit an extra and uncertain variable. For example, in obtaining an estimate of a child's "knowledge of right and wrong," results must vary according to the environment taken as a standard. "There seem to be different codes for the different situations, such as a home code, a school code, a Sunday school code, a club code" (Hartshorne *et al.,* 16). By which code shall the personality of the child be judged?

There is also confusion between personality and the factors underlying personality. Tests for physique, for intelligence, or for temperament are not tests of personality. It is not physique which acts, nor intelligence which acts, nor temperament; it is the person who acts. If, then, personality is the object of inquiry, traits of personality should not be confused with qualities or quantities of intelligence, physique, or temperament. These latter factors are merely convenient abstractions by way of which psychology has arrived at the problem of human personality. Inventories of "traits" at present include a reckless array of noncomparable factors. Kantor (20), *e.g.,* frankly employs the term to cover each and every capacity and abiding attitude of the human organism.

This practice would suggest the view that traits have merely a nominal existence. Such indeed is the attitude of Symonds (36), who regards them as dangerous fictions like the faculties of old. If "traits" are merely nominal entities, there is still the problem as to *what constitutes the existential unit of personality.* In the section which follows contributions are reviewed which bear upon the problem of the nature of this unit. For convenience it is proposed to rescue the term "trait" from the confusion in which it is embedded and to apply it consistently to designate the unit sought.

Definitions of Trait. 1. *As an independent statistical variable.* Studies using the quantitative approach tend to derive the definition approximately as follows: *A trait is a tendency to reaction which when measured with reliability demonstrates an independence of other variables.* Cady (6) objects to the use of the term "trait" in any other sense, and thinks by such delimitation a schedule may eventu-

ally be prepared of basic or generic elements in personality. Thorndike (38) says, "Let a 'single trait' be defined as one whose varying condition in men can be measured on one scale. A combination of traits requires two or more scales."

Some investigators insist upon the lack of cohesion between traits thus defined, even at a low level of organization. Thus, Trow (41) from experimental results concludes: "It would seem that little is to be gained from talking about speed of decision as a trait, if we mean by that that it is possessed in a constant amount by any one person, and used in that same quantity with slight variation on all occasions." This lack of consistency he also reports to be characteristic of confidence (40), and ventures that the same conclusion will hold for other traits as well. He professes an open mind, however, as regards certain traits, such as neatness and a sense of humor.

Working with sixth grade children, Dowd (9) made an investigation bearing upon the common contention that there is a quick type of person and a slow type. Utilizing very simple tests, she found that "rate is not to any appreciable extent an individual characteristic consistent for different performances." The only high correlations obtained were between tests having very similar content. The range of coefficients was from .136 to .870, but with a preponderance at the low end. Dowd's findings are typical of the studies which are offered as evidence for a lack of correlation between mental abilities and as support of the nonfocal theory of intelligence. The dispute concerning levels of organization in intelligence may be expected to have its counterpart in the problem of levels in personality. A few recent studies (*vide infra*) presage theories of multifocal organization in personality. For a unifocal theory it is still necessary to refer to Webb's (44) hypothesis of a "g" factor of a characterial nature.

The view that traits are specific, nonfocal reactions has, of course, long been maintained. Thorndike (38) stresses the "singularity and relative independence of every mental process, the thoroughgoing specialization of mind," and to explain the *apparent* inclusiveness of traits he leans upon the Law of Partial Activity. Symonds (36) proposes the term "confact" to designate this constant conduct response to the common element of different situations.

Perrin and Klein (32), from the approach of theory rather than experiment, claim likewise that "traits are responses conditioned upon specific stimuli," and "must be defined in terms of a particular stimulus and a particular response." Traits are transferable to other stimuli only to the extent indicated by studies of transfer of training. In this as in everything they behave like habits simply because they

are habits possessed of social significance. Traits become predictable to the extent that identities in stimulus situations are predictable.

Not all of the statistical studies can be interpreted to favor this view of specific, nonfocal response. Kelley (21) maintains that since "cleverness" (Garnett) and "social interest" (Wyman) are demonstrably related they are "probably just different aspects of a single underlying trait." This trait in turn is *demonstrably* different from, say, "persistence of motives." Kelley likewise points to aggregations of traits which suggest super-traits or type-traits. The characteristics of type-trait are: (1) that a direct or indirect quantitative measure of it exists, (2) that in a given age group it exists in amounts which correlate to the extent zero with other measured traits, (3) that this zero correlation stamps the type-trait with a peculiar value in interpreting mental life. It is interesting to note that the result of this approach is the discovery of three outstanding type-traits which parallel closely, so far as they go, Spranger's *Lebensformen* (35), viz., the social, the political, the economic.

Other constellations, of not quite so high an order, are reported. Freyd (12) finds co-variation in respect to talkativeness, flexibility, present-mindedness, good-nature, and quickness in work. Heidbreder (17) discovers that Freyd's fifty-four criteria for extroversion-introversion with few exceptions form "a consistent set of traits." The reviewer in a study yet to be published has found evidence of a similar agreement among habits which comprise the trait of ascendance (*cf.* 1).

Certain quantitative studies then, while not denying the presence of isolated habits in personality, find a tendency for them to group themselves, to adhere in complexes forming units of a higher order.

2. *As a contingent higher unit.* Other psychologists take the genetic approach. The notion of the compounding of reflexes and the doctrine of the conditioned response underlie these contributions. Sherrington and Pavlov (the latter innocently) have entrenched belief in a hierarchy of habits. When applied to personality two questions arise: (1) How high on the motor side may the hierarchy extend? (specifically, How generic may the motor attitude become through integration?). (2) How nonspecific may the stimulus become? *i.e.,* How far from the original stimulus may the effective stimulus recede? Neither question has been answered explicitly, although both are vital to the theory that traits are built through conditioning and compounding. Answers by implication are offered by the following writers:

Bridges (4) presents a thoroughgoing hierarchy consisting of

(a) hypothetical elements, (b) simple but observable elements, (c) knowledge (cognitive), minor sentiments (affective), motor habits (conative), and (d) intellectual world view (cognitive), major sentiments (affective), and a "general motor attitude" (conative). Watson (43) proposes a less extravagant hierarchy in a conditioned reflex level and a later habit level. Burnham (5) speaks of conditioned reflexes, habits, or systems of conditioned reflexes, attitudes, and generic attitudes.

Carr (7) considers that every act exhibits its own specific genetic history and also "special features which reflect some more general characteristic of the self," *e.g.*, cordiality, reserve, pride, caution, etc. The organization of such special features into higher units proceeds, he says, distinctly according to the principles of contiguity and assimilation, that is, in strict conformity with the laws of association. Paton (30) likewise claims that all complex responses which become the criteria for differentiating the behavior of one individual from that of his fellows are the operation of reaction systems gradually evolved through the integration of simpler reflexes. The origin of these systems can often be traced step by step.

From such points of approach the following approximate definition of trait might be formulated: *A trait is a dynamic trend of behavior which results from the integration of numerous specific habits of adjustment, and which expresses a characteristic mode of the individual's reaction to his surroundings.*

3. *As a noncontingent higher unit.* An attack has been launched by Humphreys (19) against the assumption that the conditioned reflex is the unit of habit. "One who reads the account given by Pavlov and his workers is forced to the conclusion that the mechanism described is qualitatively different from that which, say, James treated in his chapter on habits." The significant differences are: (1) the conditioned reflex readily disappears through disuse; (2) it is easily inhibited by slight extraneous stimulation; (3) it revives under conditions not normally influencing habit. More recently Pavlov himself (31) has shown that his endeavors to elaborate conditioned reflexes to form higher units usually lead to the inhibition of reflexes already present, to a regression to a still earlier reflex, or to diffuse cortical inhibition (sleep). To Humphreys the conditioned reflex is "a simplified type of habit rather than a unit." Mursell also (28) assembles evidence which he thinks proves that "the proposal to regard the segmental reflex as the ultimate unit of behavior" definitely breaks down. Akin to this view are the pronouncements of *Gestalt* psychology. Koffka (22) regards habit as a "closure"

resulting not from association but directly from "collective apprehension" and meaningful, motor structuration. Ogden (29) adds: "Instead of supposing that the organism is equipped at start with a number of reflexes that become integrated, or associated, by a kind of linkage into instincts and habits, we have found ample reason to believe that a total response of the entire organism precedes any differentiation of partial activities."

A view which ascribes a different sort of independence to traits is that which regards a driving power as absorbed into the higher units. From such a standpoint it is immaterial whether the higher units originated from integration; the prominent fact is that they lead an existence *sui generis*. Thus, Woodworth (45) speaks of *mechanisms becoming drives*. Troland (39), after tracing the growth of "complexes" in the child by trial and sensory pleasure and pain, points to the reciprocal phenomenon: under the control of such complexes habits are subsequently formed which are congruous with the complex itself.

Morgan (26) originally called the control exerted by the higher units over the lower "selective synthesis." He has more recently treated this phenomenon as a manifestation of emergence (27). While admitting that higher levels involve lower, he insists that, far from being functionally dependent upon them, "one may say that the higher kind of relatedness guides or controls the go of the lower level." Emergence has been applied more specifically to personality by Gordon (14): "So at whatever level we are examining personality, we must remember that the particular type of relatedness we are concerned with involves all other forms of relatedness below it. But it is not a mere summation of these, but a definite new entity of its own, different from its components and from everything else." It must, however, be urged that the doctrine of the existence of directive higher units in behavior does not depend for its validity upon the philosophy of emergence.

The conclusions to be drawn from the opinions just assembled are the following: (1) A trait probably does not reside in a specific "habit-groove" to be activated by a constant stimulus (acting as a whole or ecphorically). (2) A trait is functionally independent of its origins, even if its origins can be traced to the compounding and conditioning of reflexes (which Humphreys, Mursell, and others doubt). A trait is known not by its cause, but by what it causes; not by its roots but by its fruits. (2) Any given moment of behavior will be determined in part by the exciting stimulus, but also in part by the complex higher units in personality. This process is

spoken of by Bentley (3) as "contributory habituation," wherein nonhabituated behavior takes place under the general guidance of habits, and on the other hand habituated functions are performed in close coöperation with the specific activities aroused by the stimulus of the moment. From this point of view behavior shows a dual determination: specific and general. The latter, of course, is the type of determination manifested by traits. A trait then might be defined as *a general and habitual mode of adjustment which exerts a directive effect upon the specific response.*

The Personality as a Whole. The definition of the unit of personality is one problem pressing for solution. Equally insistent is the demand for a theory as to the way in which units are related in personality. Griffith (15) writes, "The fusion of unnumbered experiences into a single unit bearing a total character of its own or the fusion of behavior patterns of different kinds into consistent and internally coherent systems is a problem that the psychologist has barely touched. Even his generalizations about a 'common factor' are vague in the extreme. A problem lies here, however, that cannot be ignored. That totality of mental life and behavior called 'personality' or 'character' is, perhaps, the most unique thing about the human organism." The first step in the solution of this problem is to determine to what extent a personality is to be treated as an integer. If it is a true integer, then each trait *must* reflect but an aspect of a synthesis in which all components are congruous. Under such a condition it would be true of the personality, as Lavater said of the human body, that no member contradicts another, for each has the character of the whole.

Garnett (13) has sought to determine whether an index of integration can be prepared for a given personality. "A well integrated personality is one in which the various habit-systems show no marked disagreement." Using ratings as his criterion be concludes tentatively that in some individuals at least a consistency around a level is found to exist, and that estimating from the results of partial correlation abstract and social intelligence play an appreciable part in determining this consistency. Using a variety of tests, dealing with thirty-five talents and traits, Hull (18) found that the variability measured within the individual is roughly 80 per cent as great as the variability within a normal group. But individuals differ in variability, so that in some the range is twice as great as in others.

An integrated personality to Young (46) is "one which is organized around some set of facts or specific aspects of the environment,

around some values if you will." Integrity means organization. But dissociation in personality is as true a phenomenon as integration. "Dissociation is a method by which the personality utilizes one part of the organism independently of the antagonistic trends of the personality." Such dissociation is at times invaluable for survival, but ordinarily it implies a division of power and hence inefficiency. Young, then, as well as Hull and Garnett, indicate that both integration and the lack of integration are characteristics to be dealt with.

Most of the studies up to the present time have treated personality as unintegrated, attention being centered upon some trait in isolation. Few treatments of the whole personality have appeared. Twenty-four years ago Morgan (26) stated his belief that "character" evolves into a pattern "determined by the intrinsic laws of mind, just as a crystal is evolved along lines which are determined by the intrinsic laws of crystallization." This point of view has been carried forward in a brilliant exposition by Spranger (35). That unity of personality which is so apparent to the naïve eye is apotheosized by Spranger as a *Wertrichtung* under which the individual acts of a person are coördinate. Taking Spranger as a point of departure the reviewer (2) has shown that the "form-quality" of the whole personality is amenable in part to objective methods of investigation, and in part only to inferential characterization. Roback (33) like Spranger and Young finds the center of gravity in personality in the values held by the person. There is an inborn urge to consistency which forces the individual to strive toward unification in his acts. If his values are in this way achieved through the rational inhibition of instinctive impulses, the individual, according to Roback, has attained "character."

To such approaches it has been objected, as was pointed out earlier in this review, that people respond very specifically, not at all consistently, and that the personality does not represent a unit. This view is as extreme as Spranger's. One contention treats the acts of personality as nearly devoid of consecutive relation as, say, the excursions of a vehicle. The other view flatteringly ascribes an inherent (and innate) *Wertrichtung* to the poorest and most casual of personalities.

English (11) points out that behavior as the "response of a complete and integrated organism" is a fiction, since "no act is ever a response of the 'whole animal'; no organism is ever wholly integrated. Integration is a matter of degree." Spranger's method tends to disregard the disunities in personality. On the other hand, the majority of contemporary investigators pay far too little respect to

the underlying unities. *Integration in personality is a matter of degree.*

Conclusions. A consideration of the more conclusive studies suggests five basic requirements in procedures for investigating personality: (1) the recognition of "trait" as the unit of personality, (2) the admission of a probable hierarchy of traits, certainly of unit tendencies higher than the level of specific habits, (3) an approach to the problem of the limits of generalization in the most comprehensive traits, (4) the admission of both a major synthesis in personality as well as minor syntheses and dissociated acts, (5) the tentative admission of subjective values as the core of such syntheses, but the exclusion of objective evaluation (character judgments) from purely psychological method.

REFERENCES

1. ALLPORT, F. H., and G. W., Personality Traits: Their Classification and Measurement. *J. of Abnor. & Soc. Psychol.*, 1921, **16**, 6–40.
2. ALLPORT, G. W., The Study of the Undivided Personality. *J. of Abnor. & Soc. Psychol.*, 1924, **19**, 132–141.
3. BENTLEY, M., *The Field of Psychology*. N. Y.: Appleton, 1925. Pp. ix+536.
4. BRIDGES, J. W., A Theory of Personality. *J. of Abnor. & Soc. Psychol.*, 1926, **20**, 362–370.
5. BURNHAM, W. H., *The Normal Mind*. N. Y.: Appleton, 1924. Pp. xv+686.
6. CADY, V. M., The Psychology and Pathology of Personality. *J. of Del.*, 1922, **7**, 225–248.
7. CARR, H. A., *Psychology*. N. Y.: Longmans Green, 1925. Pp. iv+425.
8. DAVENPORT, C. B., The Trait Book. *Eug. Rec. Off. Bull.* No. 6, 1912. Pp. 127.
9. DOWD, C. E., A Study of the Consistency of Rate of Work. *Arch. of Psychol.*, 1926, **84**. Pp. 33.
10. EDMAN, I., *Human Traits*. Boston: Houghton Mifflin, 1920. Pp. xi+467.
11. ENGLISH, H. B., Is a Synthesis of Psychological Schools to Be Found in a Personalistic Act-Psychology? *Psychol. Rev.*, 1926, **33**, 298–307.
12. FREYD, M., The Personalities of the Socially and Mechanically Inclined. *Psychol. Mono.* No. 151, 1924, **33**. Pp. 101.
13. GARNETT, H. E., Personality as "Habit Organization." *J. of Abnor. & Soc. Psychol.*, 1926, **21**, 250–255.
14. GORDON, R. G., *Personality*. N. Y.: Harcourt Brace, 1926. Pp. xiv+298.
15. GRIFFITH, C. R., *General Introduction to Psychology*. N. Y.: Macmillan, 1926. Pp. ix+513.
16. HARTSHORNE, H., MAY, M., SONQUIST, D., and KERR, C., Testing the Knowledge of Right and Wrong. *Rel. Educ.*, 1926, **21**, 539–554.
17. HEIDBREDER, E., Measuring Introversion and Extroversion. *J. of Abnor. & Soc. Psychol.*, 1926, **21**, 120–134.
18. HULL, C. L., Variability in Amount of Different Traits Possessed by the Individual. *J. of Educ. Psychol.*, 1927, **18**, 97–106.

19. HUMPHREYS, G., Is the Conditioned Reflex the Unit of Habit? *J. of Abnor. & Soc. Psychol.*, 1925, **20**, 10–16.
20. KANTOR, J. R., *Principles of Psychology*. N. Y.: Knopf, 1924, xix+462.
21. KELLEY, T. L., Oddities in Mental Make-up. *Sch. & Soc.*, 1926, **24**, 529–534.
22. KOFFKA, K., *The Growth of the Mind*. N. Y.: Harcourt Brace, 1925. Pp. xvi+356.
23. MANSON, G. E., A Bibliography of the Analysis and Measurement of Human Personality up to 1926. *Reprint, Nat. Res. Council*, No. 72, 1926. Pp. 59.
24. MARSTON, L. R., The Emotions of Young Children. *Univ. of Iowa Stud. in Child Welfare*, 1925, **3**, No. 3. Pp. 99.
25. MAY, M., and HARTSHORNE, H., Personality and Character Tests. PSYCHOL. BULL., 1926, **26**, 395–411.
26. MORGAN, C. L., *Introduction to Comparative Psychology*. London: Walter Scott, 1908 (2nd rev. edit.). Pp. xiv+381.
27. MORGAN, C. L., *Emergent Evolution*. London: Williams and Nordgate, 1923. Pp. xii+313.
28. MURSELL, J. L., The Principle of Integration in Objective Psychology. *Amer. J. of Psychol.*, 1924, **35**, 1–15.
29. OGDEN, R. M., *Psychology and Education*. N. Y.: Harcourt Brace, 1926. Pp. xii+364.
30. PATON, S., *Human Behavior*. N. Y.: Scribners, 1921. Pp. 454.
31. PAVLOV, I. P., Le travail dès hémisphères cérébraux. *J. de Psychol.*, 1926, **23**, 501–510.
32. PERRIN, F., and KLEIN, D., *Psychology*. N. Y.: Holt, 1926. Pp. vi+375.
33. ROBACK, A. A., *The Psychology of Character*. N. Y.: Harcourt Brace, 1927.
34. ROBACK, A. A., *A Bibliography of Character and Personality*. Cambridge: Harv. Univ. Press, 1927.
35. SPRANGER, E., *Lebensformen*. Halle: Niemeyer, 1922 (3d edit.). Pp. xiii+403.
36. SYMONDS, P. M., The Present Status of Character Measurement. *J. of Educ. Psychol.*, 1927, **18**, 73–87.
37. SYMONDS, P. M., What is an Attitude? (abstract). PSYCHOL. BULL., 1927, **24**, 200–201.
38. THORNDIKE, E. L., *Educational Psychology*. N. Y.: Col. Univ. Press, 1914, 3 vols.
39. TROLAND, L. T., *The Mystery of Mind*. N. Y.: van Nostrand, 1926. Pp. xi+253.
40. TROW, W. C., The Psychology of Confidence. *Arch. of Psychol.*, 1923, **67**. Pp. 47.
41. TROW, W. C., Trait Consistency and Speed of Decision. *Sch. & Soc.*, 1925, **21**, 538–542.
42. WATSON, G. B., A Supplementary Review of Measures of Personality. *J. of Educ. Psychol.*, 1927, **18**, 73–87.
43. WATSON, J. B., *Psychology*. Phila.: Lippincott, 1919. Pp. 420.
44. WEBB, E., Character and Intelligence. *Brit. J. of Psychol., Mono. Supp.*, **1**, 3. Pp. 99.
45. WOODWORTH, R. S., *Dynamic Psychology*. N. Y.: Col. Univ. Press, 1918. Pp. 210.
46. YOUNG, K., The Integration of Personality. *Ped. Sem.*, 1923, **30**, 264–285.

STUDIES IN SEX DIFFERENCES

BY CHAUNCEY N. ALLEN

Dartmouth College

A. *Physical Differences.* Since Havelock Ellis' *"Man and Woman"* (fifth edition, 1914), the only comprehensive cataloguing of sex differences is that made by Knight Dunlap in his *Social Psychology* (1925). He divides the possibilities in a systematic fashion, thus: anatomical, physiological, and psychological differences.

Bearing upon the anatomical differences is Parsons' and Keene's (45) differentiation of the human skull, which indicates a proportionately lower and narrower female skull. Porteus and Babcock (49) have published a study which discusses sex differences in brain capacity. They agree, in general, with the findings of Burt and Moore (9). From nine to thirteen years of age the median brain capacity of boys was found to be consistently 50–60 c.c. above that of girls. Growth in brain capacity of females after fifteen years is 4.7 per cent of their ultimate adult capacity, while that of males is 8.4 per cent for the same period. The female is thus relatively more mature at fifteen years than the male, but her brain growth is slowing down. It is not to be inferred from this, however, that differences in post-pubescent growth are related to differences in learning capacity, for women show no marked inferiority when given an equal opportunity to compete with men. Brain growth is rather connected with the development of other powers, such as adaptability to life's demands, emotionality, physical hardihood, etc. Work by Bayon (4) indicates that women possess vermiform appendices that are longer and wider, and richer in fat, than those of men.

Turning to the functional aspect of physical differences, studies in the metabolism of the sexes are interesting. Three stand out in particular. Benedict and Emmes (5) and Benedict and Talbot (6) agree that metabolism for boys and men is consistently higher (by 5–6 per cent) than for girls and women, and that there is a slight increase in both sexes just after puberty. Marston (39) who has studied systolic blood-pressure in relation to criminality, concludes that men are habitually less emotional, inasmuch as they show smaller changes (two or three times as small) than women. The blood-pressure of women is much more variable under conditions of controlled quiet than that of men under like conditions. Women are

affected by many more stimuli which find reactions through the visceral segment than through the skeletal.

It is necessary to consider the real and problematical effects of endocrine secretions, especially those from the gonads, and the influence of menstruation and ovulation upon female behavior. Studies on animals give a start in this field, showing that the phenomenon of heat and the ripening of the follicles, for example, are probably effects of some deeper changes in the ovaries. Marshall and Runciman (38) give indications that the interstitial cells are most concerned in such reactions. This is another indication of the trend towards explanation of maleness and femaleness in terms of hormones.

Pezard (46) has reviewed the major work done on animals and has concluded that the testicular hormone holds as dependents a certain number of secondary sexual characteristics, action of which, beginning at puberty, is constant and continuous, and is instantly stopped with the removal of these glands. Since then the studies of Brown-Sequard on himself have come to light and have been checked by Variot, Brainard, Stanley, etc. (quoted by Stone, 60). The "father of endocrinology" succeeded in staving off the effects of senility for a short time by testicular injections, restoring in himself, at a late age, the characteristics of the vigorous male, in order to cling to his laboratory a while longer.

In all such studies, the best evidence is that the interstitial cells (both of the testes and of the ovaries) are most concerned. Stone (60) has carefully studied this field in regard to male sexual behavior, etc., in animals. His review of the literature is recommended for students.

Novak (44) has observed the interrelations that exist between the ovaries and menstruation, development of the secondary sexual characteristics, and of the thyroid, pituitary, and adrenal glands. Mosher (42), working mainly at Johns Hopkins, has contributed a valuable study which shows that the fall of blood-pressure (which occurs at definite intervals in *both* sexes) is near or at the menstrual period in women, at which time they are more than usually suggestible. She found the maximum fall to be coincident with the onset of the flow, and that a great deal of the pseudo-menstrual suffering reported was simply coincident functional disturbances in other organs.

Certain studies review the social and psychological problems and implications of menstruation. In 1914, Arnold (2) showed that much of the incapacity of girls during their menses was false, and

that exercise at that time was decidedly beneficial, rather than harmful. In England, Clow (11, 12) showed the significance of imagination and suggestion in producing the idea that menstruation must be regarded as a feminine malady, and that the time of the menses must be one of self-coddling. In interviewing 1,818 girls twice each, she raised the percentage of those who admitted they were free from any severe pain or serious indisposition from 70 per cent to 93 per cent. Other doctors concur. Stoddart (59), speaking for the psychoanalysts, even claims that *every* case of spasmodic dysmenorrhoea is an anxiety hysteria, and is curable by suggestion and an understanding of the individual case. Further, in 1923, the physicians speaking at a School Medical Officer's meeting in England (12) were in close agreement that pain would not appear if it were not expected, and that girls are generally easy to convert to this view. Bell (66) and others found that pain is relative, commonly due to under-development of the uterus, a difficulty which is helped by thyroid and ovarian extracts rather than by dilation of the uterus. Studies point in the main to the conclusion reached by Hollingworth (28): menstruation —on the great average—is far less a handicap to woman than her warped idea of its effect is, and that she performs mental and motor acts almost, if not equally, as well during her menses as when she is freed from this seeming disadvantage.

B. *Mental Differences.* The relation between mental and physical differences is shown in the results of such studies as that of Baldwin (3) on growth. He found that boys and girls are usually a year apart in their general development; that tall and heavy boys and girls are older physiologically, and mentally more mature than short, light boys and girls; and that, on the average, " weight correlates with height, +.809 for boys and +.603 for girls at any two subsequent ages from seven to seventeen." Further he indicates that the coefficients of variability are higher for boys and that physiologically accelerated boys and girls are mentally accelerated in proportion.

This study has been checked by that of Abernethy (1). He found very low partial correlations between weight and height—the commonly accepted indices of physical development—and mental age. Precocity of maturing fails to have any significant correlation with intelligence, as measured by mental tests. Mental age and height or weight correlate about zero, or very low positively. This is in direct line with a symposium edited by Thorndike (65) in which the factor of selection is shown to be the most important cause of sex differences; any theory of delayed male superiority until puberty is

unsound, for all differences are very small when allowances are made for the differential selection of subjects.

An early review by Woolley (**72**) concludes that psychological differences of sex are due to differences in the social influences brought to bear upon the developing individual from early infancy through adult years. She also edited reviews (**73, 74**) in 1910 and 1914. Hollingworth (**26, 27**) edited two reviews in 1916 and 1918. Since then, apart from the review of Burt and Moore's (**9**) study by Jones (**33**), the only survey of importance is that of Burnham (**8**), who concludes that there are no mensurable sex differences in the expression of the "social instincts," the emotional life, suggestibility, etc. A bibliography of sex differences is presented by Louttit (**36**).

Turning now to special psychological problems. Burt and Moore (**9**) found that women are superior in all forms of memory, as did Woolley (**72**). Since then it has been verified by Gates (**17a**) that men are noticeably the inferiors of women in either immediate or delayed memory. Guillett (**21**) indicates that the nature of materials used in memory work influences the results in no uncertain fashion.

A few studies of moral evaluation are recorded. Snyder and Dunlap (**57**) found a considerable similarity of judgment between men and women, with a tendency in women to assign heavier values for both the good and bad traits. Following up this same experiment, a Japanese study by Tanaka (**67**) gives essentially the same results. It is interesting to note that the greatest disagreement was on the matter of evaluating personal conduct. Again, Smith (**56**) has studied the sex life of adolescent girls. He finds that the two sexes differ in regard to views on "petting." The factor stressed is the relative cost to the members of each sex as measured by the consequences which might ensue.

In the matter of sex differences in interests, Moore (**41**) states that men are primarily interested—after the day's work is done—in money, business, and amusement, while women are primarily interested in men (sex), clothes, and decoration generally. Landis and Burtt (**35**) have repeated this experiment and published a statistical check, giving approximately the same results. Other tests of interests are here listed: Sugisaki and Brown (**62**) found that persons of the same sex were easier to remember; the Presseys (**51**) found sex differences only in likes and dislikes, moral judgments, and emotional associations; Hurlock (**32**) shows that boys and girls are differently affected by praise. The boys are markedly more respon-

sive, whereas girls, being more conscientious and interested from the start, can not raise their rate of achievement as much as the boys when incentives or rebuffs are offered. The boys were found to work below the level of their possibilities to a greater extent than the girls. Pyle (53) concludes that materials and methods of testing have a decided influence on the scores obtained in tests of ideational learning.

The problem of sex superiority may be epitomized by the reply of Dr. Johnson to the question: "Which has the most brains, man or woman?" He replied, "Which man; which woman?" Maudsley (40) states that woman reaches as high a grade of development as man, but that it is a very different type of development, conditioned by her different body. It might also be added that it is conditioned by her totally different environment and consequent interests. Pyle (52) found that girls exceed boys at all ages in learning capacity, but that the coefficients of variability were slightly greater for the boys. Hart and Olander (23) ascribe the higher grades for girls to the differential ratings (as a "sex-halo") of teachers for the two sexes. They contend that girls are much more sincere, earnest, energetic, and kind than boys, and that these traits disproportionately influence the grades given for their school work. This is in general agreement with Cunningham (13). Gowen and Gooch (20) conclude that women are equal, if not superior, to men in mental attainments in college, but they recognize the fact that women who do go to college are an even more highly selected group than the college men of to-day.

On the other hand, Uhrbrock (69), working with young children, indicates that from a large sampling of cases from nine to fourteen years old, the median score for boys was higher than that for girls in such tests as vocabulary, digit-symbols, spelling, etc. Touton (68) indicates similar results. It is significant to note that his work was in the field of geometry, however, where there is very likely a sex difference in interest and preference. Boynton (7) found no reliable differences on total scores in either the Army Alpha or the Terman tests, with a large number of cases, but rather reports that the most striking feature was the similarity of response. Again, the recently filed report of the Scholastic Aptitude Tests Commission (54) indicates that girls were superior in their tests on the scale as a whole, with a higher variability for boys, but that the unknown influence of differential selection makes the conclusions uncertain, especially since the same tests have probably differential validities for the two sexes.

The statement of the committee referred to just above is typical

of the uncertainty which characterizes conclusions on the topic of variability in general. Baldwin's (3) study on growth points to the higher coefficients of variability for boys. Ellis and others have been traditional exponents of the same thesis. There is a great deal of controversy attached to this topic. Pyle (52) found boys slightly more variable in learning capacity, except at the ages of eleven and twelve. Again, Thorndike (64) recently noted the tendency of boys to be slightly more variable. After a comprehensive study of approximately 100,000 cases, Henman and Livingstone (25) found a slightly greater variability among the boys. Hall's theory is not supported: a marked reduction in variability at adolescence as compared with childhood is found. Pressey (50), in studying 2,544 school children for intelligence, found that the total scores showed a greater variability, quite definite and convincing, for boys. It is uncertain, however, whether any permanent greater variability of male development is indicated.

On the other side of the argument we find Terman, Frasier, and Hollingworth, among others. Terman's statement that "The supposed wider variation (of intelligence) of boys is not found," is supported by Frasier (17). His results from over 60,000 thirteen-year-old boys and girls in twenty cities lead him to contradict Thorndike's statement that the "highest males in any quality are more gifted than any woman, and the lowest males are inferior to all women."

Recent data fail to controvert the earlier thesis of Hollingworth (31), that variability has no meaning unless we know just what type of variability it is we are trying to discuss. We have found nothing proved in all this work regarding *inherent* variability; even physical variability is uncertain and disputed. Social forces must be considered; there is abundant evidence to show that environments have never been the same for the two sexes. As regards mentality, data are at hand to indicate that women may be more variable in certain traits, and men more variable in others, but opinions and experiment alike leave the question still open. So far Hollingworth has no conclusive evidence to cause her to change her original statement.

In the field of abnormalities of mind, relative incidence of mental disease is to be studied with care. Hollingworth (20, 29) has two studies which indicate clearly that there are differential factors which tend to segregate the feebleminded. Girls are far less likely to be detected and brought in as social charges; they remain at large twice as long beyond the age of sixteen, and three times as long beyond the age of thirty, as do men. Healy and Bronner (24) lend support

to this view with recent statistics. Wells (71), using 100 consecutive cases of dementia precox to support this theory, shows that sex taboos are heavier in women. Finally, Pollock's (47) study of the incidence of mental disease in the United States in 1922 shows that men are strongly represented in the dementia precox and alcoholic psychosis groups, while women stand out most clearly in the manic-depressive type of insanity.

Supplementing the studies of Binet in 1903, of Gesell (18), and of Downey (15), as to the estimation of sex on the basis of handwriting, the more modern studies are as follows: Newhall (43) shows that judgments by groups may have 82 per cent agreement or better, but that experience and the nature of the group involved have a great deal to do with the results. Individual judgments were found to be much less reliable. The mean correctness in 92 subjects' judgments of 200 addresses was higher for women. Regarding handwriting as such, all of the authorities recognize inversion of "sex signs" in the handwriting specimens submitted. It is also found that the sex characteristics revealed by handwriting indicate a range all the way from positively male to just as positively female. Kinder (34) has repeated the test made by Downey and later by others—Binet's original test—more carefully controlling variables. He used, for example, twice as many judges as Binet, and half again as many as Downey employed. Binet reported 63.7 per cent correct judgments, Downey reported 67.3 per cent correct, and Kinder came within that same range with 68.4 per cent correct judgments.

In estimations of intelligence and personal traits, Webb (70) found women decidedly superior in rating women, while male ratings of women were worthless. Both sexes were about equal in the ability to judge men. Gibson (19) found that women show a very marked delayed decision time, but that they compensate with a greater accuracy. (This was a repetition of Münsterberg's "judgments test.") In judging personal characteristics, Dorcus (14) also has reported that women, on the average, take longer to form their decisions. Testing the effects of discussion on decisions, Burtt (10) found no significant indication of sex differences.

The estimation of intervals of time has been revived by experimental studies of recent date. Stewart (58) confirms earlier standard works to the effect that men are more accurate, while women tend to overestimate time. Men are more accurate in telling how much they can do of a given task—such as symbol-substitution—in a given period of time. Stewart found a slightly greater female variability. Again, Gulliksen (22) finds that women regularly over-

estimate elapsed time after periods of rest, fatigue, listening to a fast metronome, a slow metronome, and experiencing pain. The sex differences in variability were not significant. On the other hand, Swift and McGeoch (63) indicate that men's estimates were longer than those of women in eight out of twelve tests. Of these eight, however, Gulliksen, in referring back to this study, replies that only two show greater than chance differences. The weight of evidence, so far, seems to indicate that women, generally, *do* tend to overestimate time more than do men.

By way of summary, three points should be noted:

1. Few, if any, of the so-called "sex differences" are due solely to sex. Individual differences often are greater than differences determined on the basis of sex.

2. The social training of the two sexes is, and always has been, different, producing differential selective factors, interests, standards, etc.

3. The number of variables which either cannot or have not been controlled hitherto make conclusions uncertain. Among other factors, a more careful definition of terms is needed.

REFERENCES

1. ABERNETHY, E. M., Correlations in Physical and Mental Growth. *J. Educ. Psychol.*, 1925, **16**, 458–466, 539–546.
2. ARNOLD, E. H., The Effect of School Work on Menstruation. *Amer. Phys. Educ. Rev.*, 1914, **19**, 113–118.
3. BALDWIN, B. T., Relation Between Mental and Physical Growth. *J. Educ. Psychol.*, 1922, **13**, 193–203.
4. BAYON, H., Racial and Sexual Differences in the Appendix Vermiformis. *Anatom. Rec.*, 1920, **19**, 241–249.
5. BENEDICT, F. G., and EMMES, L. E., Physiology: a Comparison of the Basal Metabolisms of the Normal Man and Woman. *Proc. Nat. Acad. Sci.*, 1915, 104–105.
6. BENEDICT, F. G., and TALBOT, F. B., Metabolism and Growth from Birth to Puberty. *Carnegie Inst. of Washington*, 1921, No. 302.
7. BOYNTON, P., Sex Differences. PSYCHOL. BULL., 1926, **23**, 624–625.
8. BURNHAM, W. H., Sex Differences in Mental Ability. *Educ. Rev.*, 1921, **62**, 273–284.
9. BURT, C., and MOORE, R. C., The Mental Differences Between the Sexes. *J. Exper. Ped.*, 1912, **1**, 273–284, 355–388.
10. BURTT, H. E., Sex Differences in the Effect of Discussion. *J. Exper. Psychol.*, 1920, **3**, 390–395.
11. CLOW, S., The Effects of Physical Exercise on Menstruation. *Lancet*, 1923, **204**, 1161–1162.
12. CLOW, S., Unwell. *Lancet*, 1923, **204**, 1219–1220.
13. CUNNINGHAM, A. B., The Sex Element in College Grades. *Sch. & Soc.*, 1921, **14**, 523–525.

14. Dorcus, R. M., Some Factors Involved in Judging Personal Characteristics. *J. Appl. Psychol.*, 1926, **10**, 502–518.
15. Downey, J. E., Judgments of the Sex of Handwriting. *Psychol. Rev.*, 1910, **17**, 205–216.
16. Frasier, G. W., Sex Differences in Mental Traits and School Achievement. 1919. (*M.A. thesis; unpublished.*) Leland Stanford Univ.
17. Frasier, G. W., A Comparative Study of the Variability of Boys and Girls. *J. Appl. Psychol.*, 1919, **3**, 151–155.
17a. Gates, A. I., Experiments on the Relative Efficiency of Men and Women in Memory and Reasoning. *Psychol. Rev.*, 1917, **24**, 139–146.
18. Gesell, A. L., Accuracy in Handwriting as Related to School Intelligence and Sex. *Amer. J. Psychol.*, 1906, **17**, 394–405.
19. Gibson, L. M., A Decision Study of 150 Young Men and Women. *J. Appl. Psychol.*, 1920, **4**, 364–374.
20. Gowen, J. W., and Gooch, M., Age, Sex, and the Interrelations of Mental Attainments of College Students. *J. Educ. Psychol.*, 1926, **17**, 195–207.
21. Guillett, C., A Study of the Memory of Young Women. *J. Educ. Psychol.*, 1917, **8**, 65–84.
22. Gulliksen, H., The Influence of Occupation on the Perception of Time. *J. Exper. Psychol.*, 1927, **10**, 52–59.
23. Hart, H., and Olander, E., Sex Differences in Character as Indicated by Teachers' Ratings. *Sch. & Soc.*, 1924, **20**, 381–382.
24. Healy, W., and Bronner, A. F., *Delinquents and Criminals, Their Making and Unmaking.* 1926. N. Y.: Macmillan. Pp. viii+317.
25. Henman, A. C., and Livingstone, W. F., Comparative Variability at Different Ages. *J. Educ. Psychol.*, 1922, **13**, 17–29.
26. Hollingworth, L. S., Sex Differences in Mental Traits. Psychol. Bull., 1916, **13**, 377–383.
27. Hollingworth, L. S., Comparison of the Sexes in Mental Traits. Psychol. Bull., 1918, **15**, 427–432.
28. Hollingworth, L. S., *Functional Periodicity: an Experimental Study of the Mental and Motor Abilities of Women During Menstruation.* 1914. N. Y.: Teachers College, Columbia University. Pp. 101.
29. Hollingworth, L. S., Frequency of Amentia as Related to Sex. *Med. Rec.*, 1913, **84**, 753–756.
30. Hollingworth, L. S., Differential Action Upon the Sexes of Forces which Tend to Segregate the Feeble-minded. *J. Abn. Psychol.*, 1922, **17**, 35–57.
31. Hollingworth, L. S., Variability as Related to Sex Differences in Achievement. *Amer. J. Soc.*, 1914, **19**, 510–530.
32. Hurlock, E. B., The Value of Praise and Reproof as Incentives for Children. *Arch. of Psychol.*, 1924, No. 71.
33. Jones, G. E., Mental Differences Between the Sexes. *Ped. Sem.*, 1913, **20**, 401–404.
34. Kinder, J. S., A New Investigation of Judgments on the Sex of Handwriting. *J. Educ. Psychol.*, 1926, **17**, 341–344.
35. Landis, M. H., and Burtt, H. E., A Study of Conversations. *J. Compar. Psychol.*, 1924, **4**, 81–89.
36. Louttit, C. M., A Bibliography of Sex Differences in Mental Traits. *Tr. Sch. Bull.*, 1925, **22**, 129–138.
37. Macaulay, E., Some Social, Age, and Sex Differences Shown in Children's Choice of Ideals. *Forum of Educ.*, 1925, **3**, 105–114.

38. MARSHALL, F. H. A., and RUNCIMAN, J. G., On the Ovarian Factor Concerned in the Recurrence of the Oestrous Cycle. *J. Physiol.*, 1914, **49**, 17–22.

39. MARSTON, W. M., Sex Characteristics of Systolic Blood Pressure Behavior. *J. Exper. Psychol.*, 1923, **6**, 387–419.

40. MAUDSLEY, H., Sex in Mind and Education. *Educ. Rev.*, 1918, **55**, 427–437; **56**, 158–168.

41. MOORE, H. T., Further Data on Sex Differences. *J. Abn. Psychol.*, 1922, **17**, 210–214.

42. MOSHER, C., Normal Menstruation and Some of the Factors Modifying It. *Johns Hopkins Hosp. Bull.*, 1901, **12**, 178–179.

43. NEWHALL, S. M., Sex Differences in Handwriting. *J. Appl. Psychol.*, 1926, **10**, 151–161.

44. NOVAK, E., The Rôle of the Endocrine Glands in Certain Menstrual Disorders. *Endocrinol.*, 1920, **4**, 411–419.

45. PARSONS, F. G., and KEENE, L., Sex Differences in the Skull. *J. Anat.*, 1919, **54**, 58–65.

46. PEZARD, A., Secondary Sexual Characteristics and Endocrinology. *Endocrinol.*, 1920, **4**, 527–546.

47. POLLOCK, H. M., Mental Disease in the United States in Relation to Environment, Sex, and Age. 1922. *Amer. J. Psychiat.*, 1925, **5**, 219–232.

48. POLLOCK, H. O., Frequency of Dementia Praecox in Relation to Sex, Age, Environment, Nativity, and Race. *Ment. Hyg.*, 1926, **10**, 596–611.

49. PORTEUS, S. D., and BABCOCK, M. E., *Temperament and Race.* 1926. Boston: Badger. Pp. xiv+364.

50. PRESSEY, L. W., Sex Differences Shown by 2,544 School Children on a Group Scale of Intelligence with Special Reference to Variability. *J. Appl. Psychol.*, 1918, **2**, 323–340.

51. PRESSEY, L. W., and PRESSEY, S. H., Further Data with Regard to Sex Differences. *J. Appl. Psychol.*, 1921, **5**, 78–84.

52. PYLE, W. H., Sex Differences and Sex Variability in Learning Capacity. *Sch. & Soc.*, 1924, **19**, 352.

53. PYLE, W. H., The Relation of Sex Differences to Kind of Matter Used. *J. Educ. Psychol.*, 1925, **16**, 261–264.

54. *First Annual Report,* Commission on Scholastic Aptitude Tests. The 1926 Tests. New York: Col. Entr. Exam. Bd., 1926. Pp. 38.

55. SEVERSON, S. O., The Relation of the Anatomical Age to the Chronological, Physical, and Mental Ages, with Special Reference to Sex Differences. *J. Psychasth.*, 1922, **27**, 150–170.

56. SMITH, G. F., Certain Aspects of the Sex Life of the Adolescent Girl. *J. Appl. Psychol.*, 1924, **8**, 347–349.

57. SNYDER, A., and DUNLAP, K., A Study of Moral Evaluations by Male and Female Students. *J. Compar. Psychol.*, 1924, **4**, 289–324.

58. STEWART, I. C., Sex Differences in Substitution and Time Estimation. *J. Compar. Psychol.*, 1926, **6**, 243–265.

59. STODDARD, W. H. B., Spasmodic Dysmenorrhoea. *Lancet*, 1923, **204**, 925.

60. STONE, C. P., Experimental Studies of Two Important Factors Underlying Masculine Behavior, etc. *J. Exper. Psychol.*, 1923, **6**, 85–106.

61. STRONG, J., Some Sex Differences and the School. *Forum of Educ.*, 1923, **1**, 115–121.

62. SUGISAKI, Y., and BROWN, W., The Correlation Between Sex of Observers and Sex of Pictures Recognized. *J. Exper. Psychol.*, 1916, 1, 351–354.
63. SWIFT, E. J., and MCGEOCH, A. J., An Experimental Study of the Perception of Filled and Empty Time. *J. Exper. Psychol.*, 1925, 8, 240–249.
64. Symposium: (Ed. by Thorndike, E. L.) On the Variability of Boys and Girls from 13 to 18. *Ped. Sem.*, 1926, 33, 182–184.
65. Symposuim: (Ed. by Thorndike, E. L.) Sex Differences in Status and Gain in Intelligence Scores from 13 to 18. *Ped. Sem.*, 1926, 33, 167–181.
66. Symposium: Intrinsic Dysmenorrhoea. *Lancet*, 1923, 204, 845–849.
67. TANAKA, S., A Study of Moral Evaluation by Boys and Girls. *Japan J. Psychol.*, 1926, 1, 81–99. (Rev.: *Psychol. Abstr.*, 1927, 1, 93.)
68. TOUTON, F. C., Sex Differences in Geometric Abilities. *J. Educ. Psychol.*, 1924, 15, 234–247.
69. UHRBROCK, R. S., Sex Differences as Noted in Grammar School Children. *Sch. & Soc.*, 1920, 11, 507–509.
70. WEBB, L. W., Ability in Men and Women to Judge Intelligence. *Sch. & Soc.*, 1924, 20, 251–254.
71. WELLS, F. L., *Mental Adjustments.* N. Y.: Appleton, 1917. Pp. 320.
72. WOOLLEY, H. T. (Thompson, H. B.), *The Mental Traits of Sex.* Chicago Univ. Press, 1903. Pp. vii+188.
73. WOOLLEY, H. T., Review of Recent Literature on the Psychology of Sex. *Psychol. Bull.*, 1910, 7, 335–342.
74. WOOLLEY, H. T., The Psychology of Sex. *Psychol. Bull.*, 1914, 11, 353–379.

W. H. R. RIVERS. *Psychology and Ethnology* (Edited with Preface and Introduction by G. Elliot Smith). N. Y.: Harcourt, Brace. 1926. xxviii+324 pp.

This is a collection of papers by the late Dr. Rivers covering a wide range of topics, only a few of which have any direct psychological import. Although many of them have appeared previously in various publications, it is convenient to have them brought together in one collection.

It is presumably the last collection of addresses and papers by Dr. Rivers, and Dr. Smith takes occasion to summarize the contributions which this renowned British anthropologist has made. It will be fitting, therefore, to assess here Rivers' major contributions to anthropology.

The character of Rivers' work falls into a few chronological periods, which follow in the main a logical order. He started his career as a medical man and was soon interested in the psychological work of his contemporaries, first in abnormal psychology, then in the experimental "brass instrument" psychology which was popular thirty years ago, and more, perhaps, than any other one man was responsible for giving psychology a foothold in British universities. His first anthropological work was testing the sense discriminations of savages by the laboratory methods then in vogue. His tests of Egyptians, Sholagas, Todas, and the peoples of the Torres Straits convinced him that there was no fundamental difference in the sense discriminations of men of the lowly cultures and those of civilization, save perhaps in discrimination of blue and shades of blue. His work in this field is of a high order of merit and has stood the test of subsequent decades.

He then turned attention to objective methods of analyzing primitive culture and developed his deservedly famous genealogical method. This consisted of recording the names of the individuals in a community, their relationships, and the native terms of kinship. This was a great advance upon the methods of investigation then commonly used by students in the field and it had many far-reaching advantages.

His next step was to attempt to analyze cultures by an objective method which would give an insight into their historical background.

Working on the hypothesis that culture traits in a given phase of social life, such as funeral rites, are uniform in a given culture so long as outside influences are excluded, he analyzed cultures to determine what elements were introduced and whence they came. If an Australian tribe has several methods of disposing of the dead, then some of these, he said, have been introduced. If adjacent tribes in Australia practise different funeral rites, though the substratum of the cultures is similar, then funeral rites have been introduced from an outside source. If they have been introduced an immigrant people brought them, and so a mixture of cultures implies a mixture of peoples. Hence his interest in diffusion of culture and the attempt to trace diffusion to its source. By that time Elliot Smith and Perry were in the field with their hypothesis that culture had spread from Egypt to the remainder of the world, particularly to Indonesia and Polynesia, and Rivers welcomed the hypothesis almost as if it were a demonstration. It is in the last phase of his work, the inferences as to diffusion, contacts of peoples, and immigration of populations, that most anthropologists take issue with Rivers, Smith and Perry being almost his only protagonists in this field of interpretation. The reviewer is among those who regard the last phases of Rivers' work as the weakest, as an illustration of method uncritically developed and uncritically applied.

In his tests of the psychological abilities of savages, Rivers had a check upon his observations and his conclusions, for the results were compared with similar experiments upon Caucasoids. In the genealogical method inference could be checked by the varied data under observation, so that practically all of the results could be given as verified observations. But in his inference as to contacts of peoples there are unverified hypothesis and unverified conclusions. The data which he presents as verification are such only if one takes for granted the very point which they are adduced to prove. He gives a historical orientation to the phenomena which is justifiable only if his major premise is true, and he has nowhere endeavored to establish the truth of that premise but has been content merely to state it.

Take, for example, the matter of disposing of the dead, an instance which Rivers adduces in at least three of his papers. The fact is that in many parts of the world which are, so far as we know, free from immigrant influences, there are great variations in the manner of disposing of the dead. Moreover, in a given tribe the method often differs with the social status of the deceased. Thus in Australia, an area which he cites, the bodies of women are disposed of in one manner, those of men in another, those of warriors may be given

special treatment, those of children are treated differently from those of adults. In each case the ceremony reflects the social status of the deceased. Rivers elsewhere acknowledges the correlation of type of funeral rite with social status but supposes it due to the fact that invaders were of a different social status than the earlier peoples and superimposed their rites upon those already in vogue. But in the areas which he uses there is not a shred of evidence in support of this (rather than of some other) hypothesis, and granted that such superimposition sometimes takes place, it does not follow that it is a universal explanation. The facts prove his contention only if the truth of the premise which he uses is taken for granted.

One of the most important results of Rivers' early psychological work was his conclusion that psychology cannot solve the problems of culture. He had returned from his study of the Todas with the conviction that the human mind is everywhere fundamentally the same, and that culture development, therefore, can be explained in terms of psychology. He finished with the conviction that only an analysis of culture can solve the problems of culture. In this he was, we believe, profoundly correct, for even if the human mind were everywhere the same culture could be everywhere different; and the fact is that from culture analysis we can infer the types of psychology prevalent in various ethnographical regions. The conditions which elicit revenge, fear, or sympathy are not everywhere the same. Acts which are reprobated in one region are praised in another, those which inspire revenge in one culture elicit only indifferent attention in another. Granted that men are alike in native endowments, that merely means that we know how they will react if we know the culture in which they live. We cannot similarly anticipate the culture by analyzing their native endowments. There is, then, a self-sufficiency in culture so far as culture phenomena are concerned, whereas no insight into individual psychology will enable one to predict the nature of the culture. It is interesting to see the development of this point of view in an anthropologist who was in the beginning and remained to the end profoundly interested in psychological problems.

It is mainly for the appreciation of the importance of culture facts for culture analysis, for the work in experimental psychology, and for the genealogical method that Dr. Rivers will be gratefully remembered by anthropologists. His later methodology was based on assumptions which he did not endeavor to establish and his conclusions fall or stand as his assumptions are substantiated or are shown to be useless or false. His keen mind seemed entirely closed to an appreciation of the character of the assumptions which he was

making. For example, he tells us that "the transitions which have been taken to be evidence of independent processes of evolution based on psychological tendencies common to mankind are by the modern German school ascribed to the mixture of cultures and of peoples. Further, similar patterns, even one so simple as the spiral, when found in widely separated regions of the earth, are held to have been due to the influence of one and the same culture" (p. 124). After thus stating the thesis that where similarities in culture occur they are due to diffusion and not to independent origins, he goes on, after two short paragraphs, to say: "The situation is one which has an especial interest for me in that I have been led quite independently to much the same general position as that of the German school by the results of my own work in Oceania with the Percy Sladen Trust Expedition. With no knowledge of the work of this school, I was led by my facts to see how much, in the past, I had myself ignored considerations arising from racial mixture and the blending of cultures, and I will briefly sketch the history of my own conversion" (p. 125). No similarities are possible save those due to diffusion, and he had come to the same conclusion on this matter as the German school quite independently of them! Was that remarkable similarity in their respective views due to the racial mixture of Rivers and Graebner, or to the blending of their cultures? He says it must be one or the other but then forthwith declares that it is neither. The man who can say these things in the same breath is certainly dominated by his principle and is not dominating it. This example, I think, is not unfair to Rivers. He seems to imply throughout his argument about contact of culture that if a principle might be true, and if one finds facts consonant with it, then it is true.

But grant this assumption and one can prove almost anything. Grant that since the beginning of human history man has been aspiring to rise above the lowly earth toward the skies and one can explain the erect stature of man, the greater height of the more aspiring male, the abandonment of caves, rock shelters, subterranean abodes, the taking to horseback and to conveyances which keep the feet off the ground, the putting of floors into houses, the piling up of higher stories, the building of elevated roads, airplanes, and I know not what. I am sure that one could make out as good a case for this principle as Rivers makes out for his contacts of peoples and his diffusion of culture from Egypt and farthest Ind to Tonga and the Andes. It is fairly easy to prove a principle if one admits that it is true. But I do not find that Rivers' position differs in that respect from some of the other so-called scientific anthropology which flour-

ishes at the present day. For who likes to call one's method anything other than "scientific" and "historical"?

WILSON D. WALLIS

A. A. ROBACK. *A Bibliography of Character and Personality.* Cambridge: Harv. Univ. Press, 1927.

Dr. Roback's bibliography is the second extensive survey of literature in the field of personality to appear within the past year. It offers interesting points of comparison with Dr. Manson's work (*A Bibliography of the Analysis and Measurement of Human Personality up to 1926,* Washington: Nat. Res. Council, Reprint No. 72, 1926). The first arresting point of comparison is the relatively slight overlapping. Less than 15 per cent of the total number of citations are duplicated in the two bibliographies. The lists are complementary, and the investigator will find it advantageous to have both at hand for reference. Altogether over 3,000 separate references are listed. Neither bibliographer claims to have exhausted the field, for each had a unique purpose in view during the compilation. Miss Manson wished "to assist workers in preparing reliable and valid measures of personality"; Dr. Roback wished to document his formal treatise on character (*The Psychology of Character,* New York: Harcourt, Brace, 1927).

Another point of comparison lies in the use of foreign articles. Close to 60 per cent of Dr. Roback's references are from publications in a foreign language, approximately 50 per cent of the total being in German. Dr. Manson has given a scant 8 per cent of her space to foreign contributions. Here again Dr. Roback is searching broadly for material relevant to the comprehensive treatment of character given in his book. "Characterology must be recognized as a synthesis of a number of points of view." Dr. Manson has devised her work primarily for testers, and American testers do not take kindly to the sophistry and formalism of European characterology, nor, unfortunately, to the nutritious first principles of character study sometimes found in foreign work.

Occasionally Dr. Roback annotates his references, especially where they bear some special relation to the argument of his book. Each reference is accompanied by a symbol which indicates the nature of its approach to the problem of personality. There are thirty-two such approaches distinguished, including the psychological, psychoanalytic, ethical, sociological, etc. One appendix is devoted to a reclassification of titles according to point of view. In his efforts

to be up to date the author has included another appendix of unpublished works and works in preparation.

A chronological Table of Characterology is added which throws into relief the outstanding events in the field from the time of Empedocles. This device, together with the classification of titles, makes the book useful as a guide to reading for students.

Bibliographers perform an arduous and thankless task, but the fruits of their labor are permanent. Dr. Roback's work is particularly commendable since it makes accessible a knowledge of standard works, no matter of what antiquity or nationality. It thus broadens the horizon of the student beyond mere superficial interest in testing. A wise investigator in personality needs to work backward and across the seas, and Dr. Roback's bibliography will help him. Paradoxically enough, such excursions are often the surest road forward.

G. W. ALLPORT

DARTMOUTH COLLEGE

WILSON D. WALLIS. *An Introduction to Anthropology.* N. Y.: Harper, 1926. xvi+520 pp.

The writer of a modern text in anthropology is forced to choose between the two horns of what really is an insuperable dilemma. Either he must attempt to sketch all phases of the subject, including the bewildering and ever-accumulating array of material in the field of culture, or he must confine attention to a few phases of his field, developing these in some detail, but omitting entirely any consideration of many other equally important aspects of the subject. There is no half way about it—it is either, or. And no matter what may be the choice, the author is certain to be criticized. If he elects the broad survey, there will be those who declare that a few pages each devoted to art, music, social organization, etc., are worse than useless. If he chooses to develop fully certain topics, the cry will be raised that others were more important, and that through selection a most incomplete picture has been given to the student.

Professor Wallis has decided upon the broad picture, and in his recent book attempts to present, even if only in merest fragments, a complete survey of anthropological material. The result is that certain subjects are admittedly most briefly discussed: the position of women in primitive society, in eight pages; language, in fifteen pages; mythology, in three pages; religion, in fourteen pages, and so on through other phases of primitive life. However, it seems only fair to judge the book as a whole, and upon its sum total effect. What kind of a picture will it leave in the mind of the student?

Compact as it is, it can be said that a reading of the book will probably, with the aid of the teacher, give the student a fairly comprehensive view of the subject, and provide him with the minimum background upon which he can build in more detail later. Professor Wallis has written a textbook, and it must be judged as such. It is to be assumed that many of the topics will be elaborated in the classroom by the instructor, or by the student through further reading in the references which the author has included.

The book is divided into five parts: physical anthropology; prehistoric man; economic and industrial activity; science, magic, and religion; and social morphology and culture. The author has combined his theoretical and descriptive material skilfully, and has presented the theoretical matter in a manner that makes its significance clear to the undergraduate. Professor Wallis himself holds the views of the American school of anthropologists, and may be classed with the cultural determinists. A reading of the book will give anyone a concise statement of the general position of the American anthropologists. The author's own theories do not intrude, however, upon the text. For the teacher, the volume should prove a useful book around which to build an introductory course in anthropology.

MALCOLM M. WILLEY

DARTMOUTH COLLEGE

NOTES AND NEWS

AT the meeting of the British Association for the Advancement of Science held late last summer, William Brown was elected sectional president of Section J (Psychology).

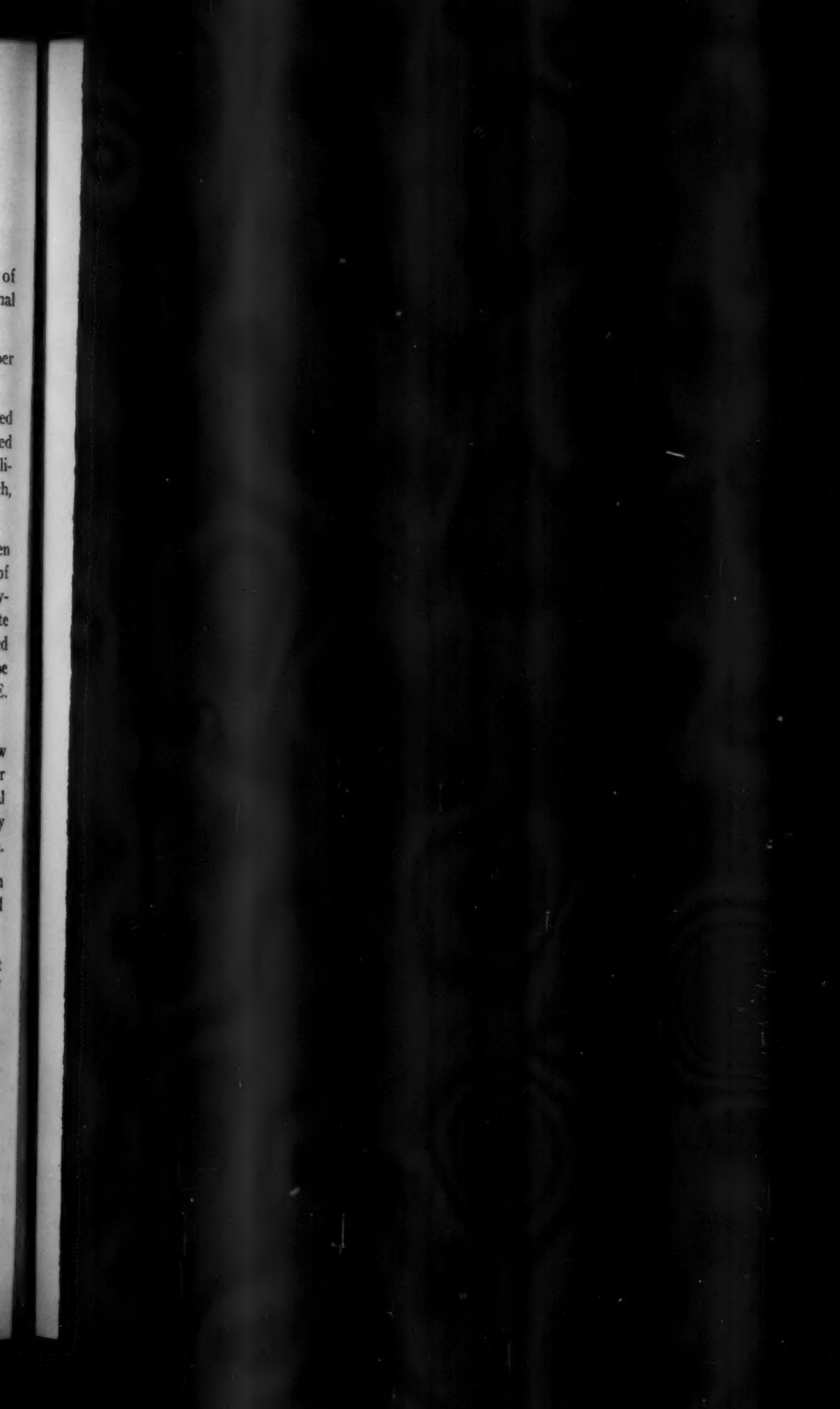
THE death is announced of Professor Emil Kraepelin on October 7, 1926.

DR. BERNARD GLUECK, psychiatrist, who has recently returned from a year and a half spent abroad, where he has been associated with the European psychiatrists, is to give a course on "The Applications of Psychoanalysis" at the New School for Social Research, New York City, during the spring term.

THREE good fellowships in the psychology of music have been made available at the University of Iowa by Mr. George Eastman of Rochester, N. Y., for the purpose of studying the application of psychological technique to problems in the teaching of music. Graduate students applying for these appointments should present advanced training in both music and psychology. The appointments will be made April 1. Applications should be made to Professor C. E. Seashore.

DR. LEE TRAVIS, for three years National Research Council Fellow in psychology and psychiatry, has been appointed assistant professor of clinical psychology and psychologist to the Psychopathic Hospital in the State University of Iowa. His work will be devoted mainly toward the direction of research within the field of speech correction.

THE title of professor of psychology in the University of London has been conferred on Dr. Beatrice Edgell, in respect of the post held by her at Bedford College.

DR. HORACE B. ENGLISH, associate professor of psychology at Wesleyan University, has resigned to take a position as professor of psychology at Antioch College.

PROFESSOR CHARLES HENRY, late director of the laboratory of physiology of sensations at the Sorbonne, died on November 9 after a long illness.